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ILLUSTRATED WITH ENGRAVINGS.

BY WILLIAM NICHOLSON.

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THE ARTS.

APRIL 1801.

ARTICLE I.

Some Observations on different Hydrocarbonates and Combinations of Carbone with Oxygen, &c. in reply to some of Dr. Priestley's late Objections to the New System of Chemistry. By W. CRUICKSHANK, *Woolwich.*

ON perusing Dr. Priestley's late publication, in which he supports the doctrine of phlogiston, I was much struck with some of the experiments there related, more especially with the quantity and nature of the gases obtained from the grey oxide of iron and charcoal, and the same oxide with the carbonate of barytes.

Although I had not the least doubt of the accuracy of these experiments, yet I was determined to repeat, and extend them, and to pay particular attention to the nature of the different gases obtained.

Accordingly a quantity of the grey oxide, or scales of iron separated in forging, were introduced into a crucible, and exposed to a red heat for upwards of half an hour; at the same time a quantity of powdered charcoal was introduced into another crucible, which had a cover luted on, and likewise exposed to a strong red heat; these were mixed while hot, and introduced into a small coated glass retort. The retort was then placed in a furnace, its neck being plunged under water, in a pneumato-chemical tub, and heat gradually applied.

By the time that the retort became red, or rather sooner, gas was produced in great abundance, and continued to come over for two hours or upwards; this gas was received in different veſſels, ſo that the portions obtained at different periods of the proceſs might be examined ſeparately: the whole amounted to 150 oz. meaſures (troy.)

The firſt portion conſiſted of carbonic acid gas, one part, and inflammable gas four parts.

The ſecond portion of carbonic acid gas, one part, and inflammable gas five parts.

The third was exactly ſimilar to the ſecond, and then to the end of the proceſs; it conſiſted pretty uniformly of carbonic acid gas, one part, and inflammable gas, ſix parts. I repeated this experiment, and employed an iron retort, in order that a much greater degree of heat might be applied. In this caſe the gaſes obtained were exactly ſimilar, and the very laſt portion was found to conſiſt of carbonic acid gas one part, and inflammable gas ſix parts; but the quantity in all, although the oxide of iron and charcoal together did not exceed two ounces, was prodigious, and muſt have amounted to many gallons, by meaſure.

Suſpecting that ſome of the other metallic oxides might yield ſimilar reſults, if treated with charcoal in the ſame manner; I expoſed a quantity of the ſublimed oxide of zinc to a red heat for nearly an hour, and then mixed it with charcoal, which had likewiſe been previouſly heated; theſe were introduced into a coated glaſs retort, and ſubjected to a heat gradually raiſed as in the former experiments; before the retort became red, a conſiderable quantity of gas was diſengaged, but upon increaſing the heat it came over in torrents.

The firſt portion conſiſted of carbonic acid gas, and inflammable gas, in the proportion of 1 to 9. The ſecond, of the ſame gaſes, in the proportion of 1 of the former, to 26 of the latter, and what was received afterwards, was purely inflammable; the whole amounted to 90 ounce meaſures. At the end of the proceſs a quantity of metallic zinc was found in the neck of the retort.

I next ſubjected the red oxide of copper and charcoal to a ſimilar proceſs, and the quantity of gas collected from an ounce of this mixture, amounted to 64 ounce meaſures.

The firſt portion conſiſted of the carbonic acid, and inflammable gaſes, in the proportion of 10 of the former to one of the latter. The ſecond of the ſame gaſes, in the proportion of 15 to 55; and the third was purely inflammable gas, without any ſenſible quantity of carbonic acid. At the end of the proceſs the copper was found in ſmall globules in its metallic ſtate, mixed with the charcoal.

The next ſubject of experiment was litharge, which was dried as much as poſſible, mixed with hot charcoal, then introduced into a coated glaſs retort, and expoſed to heat in the ſame manner as the other mixtures.

The firſt portion of gas conſiſted of carbonic acid, and inflammable gas very nearly in equal bulks. The ſecond, of the ſame gaſes in the proportion of 13 of the former to 39 of the latter, and what came over afterwards was almoſt purely inflammable, the whole amounting to about 40 ounces. The lead was found revived and mixed with the charcoal in the form of ſmall globules.

The

The laſt metallic oxide which I ſubjected to this proceſs, was that of manganeſe; about half an ounce of the black ore of this ſemi-metal was expoſed for an hour to a ſtrong red heat in a crucible, it was then mixed with hot charcoal, and introduced into a coated glaſs retort; heat being applied and gradually increaſed, a quantity of gas, amounting in all to 38 ounces, was procured. The firſt portion was chiefly carbonic acid; the ſecond a mixture of this with inflammable gas, in nearly equal proportions; and the laſt almoſt purely inflammable.

From theſe experiments we may draw the following concluſions: 1ſt. That all metallic oxides capable of enduring a red heat, will, when mixed with charcoal, not only yield carbonic acid, but alſo a very conſiderable quantity of an inflammable gas. 2dly, That thoſe oxides which retain their oxigen moſt obſtinately, yield the greateſt quantity of inflammable gas; and on the contrary, that thoſe which part with it readily, afford the greateſt proportion of carbonic acid. And 3dly, That the carbonic acid gas was chiefly diſengaged at the beginning of the proceſs, and the pureſt and greateſt quantity of inflammable gas towards the concluſion of it.

Having thus aſcertained the relative proportions of theſe gaſes, I next endeavoured to inveſtigate the properties of the inflammable gas, which from the peculiar circumſtances under which it was procured, I had little doubt muſt be different from any of the known hydrocarbonates.

Accordingly I took a quantity of that obtained from the grey oxide of iron, and waſhed it repeatedly with lime water, until the whole of the carbonic acid was completely ſeparated. Its ſpecific gravity was then aſcertained, and it was found to be but a very little lighter than common air, that is to ſay, in the proportion of about 22 to 23, a circumſtance which proved it at once to be different from the common hydrocarbonates, all of which are much lighter than common air.

I next endeavoured to eſtimate the quantity of pure oxygen neceſſary to ſaturate it, and likewiſe the products of the combuſtion. After ſeveral trials I found, that when four meaſures of the waſhed gas, mixed with two of oxygen, were introduced into a ſtrong glaſs jar over mercury, and fired by the electric ſpark, they were reduced to $3\frac{1}{5}$ meaſures, and that when this reſiduum was agitated with lime water, there only remained about $\frac{1}{4}$ of a meaſure, which was found by the nitrous teſt to be pure oxygen; hence it would appear, that eight meaſures of this gas require $3\frac{1}{2}$ meaſures of pure oxygen gas to ſaturate them, and when combined produce nearly ſix meaſures of carbonic acid gas, with a little water. Now if we ſuppoſe a meaſure equal to a cubic inch, and that a cubic inch of the inflammable gas, at the temperature of 55°, and under a mean preſſure of the atmoſphere, weighs .3 of a grain, a cubic inch of oxygen gas, .34 of a grain, and a cubic inch of carbonic acid gas .47 of a grain; then the weight of eight meaſures of the inflammable, with $3\frac{1}{2}$ of oxygen gas, will be 3.6 grains very nearly; but the weight of the carbonic acid gas generated, is only 2.8 grains; hence there remains a deficiency of .8 of a grain to be accounted for, and which muſt ariſe from the water produced. The chief circumſtance, however,

which

which diſtinguiſhes this gas from other inflammable gaſes, is the great proportion of carbonic acid which it yields when united with oxygen. In the above experiment we find ſix meaſures of carbonic acid gas generated; now this quantity would require at leaſt ſeven meaſures of pure oxygen, whereas the quantity employed was only 3½ meaſures; we muſt therefore infer, that the remaining 3½ meaſures muſt have been originally combined with the inflammable gas, conſtituting what may be called a gaſeous oxide. Hence it would appear, that this gas bears the ſame relation to carbonic acid, that nitrous gas does to the nitrous acid. In future, for the ſake of diſtinction, we ſhall call it the gaſeous oxide of carbone. The gaſes obtained from the other metallic oxides and charcoal, were perfectly ſimilar in their properties; all of them when mixed with common air and ignited, burned with a lambent blue flame, without any ſenſible exploſion, and much carbonic acid, with a little water, was produced. When mixed with nitrous gas no diminution or ſenſible charge could be perceived, which proves that their oxygen is in a combined, and not in a diſengaged ſtate.

Dr. Prieſtley has likewiſe found, that when the carbonate of barytes was mixed with the grey oxide of iron, and expoſed to a ſtrong heat in a retort, that not only carbonic acid, but alſo inflammable gas was obtained. In order to ſatisfy myſelf with regard to this fact, I mixed a quantity of the carbonate of barytes and oxide of iron, both having been ſeparately made red hot for ſome time, and introduced them into a dry coated glaſs retort. Heat being gradually applied, a quantity of gas, which at firſt was nothing more than carbonic acid mixed with a little azote, eſcaped; but when the retort had been red for a few minutes, the gas diſengaged conſiſted of carbonic acid 25 parts, and gaſeous oxide of carbone ten parts; the whole amounting to 30 ounce meaſures. This experiment was repeated, and an iron retort employed, in order that a much greater degree of heat might be given; in this caſe, not leſs than 90 ounce meaſures of a ſimilar mixture of gaſes were obtained.

Conceiving that in theſe experiments the gaſeous oxide muſt proceed from the partial decompoſitions of the carbonic acid by the iron, when raiſed to a high temperature, I thought I ſhould ſucceed better by employing iron filings in place of the grey oxide, as theſe would have a greater affinity for oxigen. Accordingly I took a quantity of common chalk, and expoſed it to a low red heat for 10 minutes; it was them mixed with an equal quantity of clean iron filings made as dry as poſſible; the mixture being introduced into an iron retort, heat was gradually applied. As ſoon as the retort became red, gas began to be diſengaged in great abundance, and continued to come over for a conſiderable length of time, ſo that the whole muſt have amounted to many gallons. Portions of this being examined at different periods, it was found to conſiſt on a medium of one part of carbonic acid gas, and four or five parts of gaſeous oxide of carbone.

From this experiment we perceive, that a much greater proportion of gaſeous oxide is obtained, and more carbonic acid decompoſed, where iron filings are employed in place of the grey calx, which proves that it is the acid which is decompoſed by the iron. In an-

other

other inſtance I ſubſtituted lime for the carbonate of lime ; here too the gaſeous oxide was obtained, but in much leſs quantity, and had the lime been perfectly pure, it is probable that very little, or none, would have been procured; this experiment likewiſe points out the origin of the gas. In inveſtigating the properties of this inflammable gas, I found that when perfectly free from carbonic acid, it was lighter than common air, in the proportion of 22 to 23.

That when mixed with common air and ignited, it did not explode, but burnt ſlowly with a lambent blue flame : that when the products of this combuſtion were collected, by burning a conſiderable quantity in a large receiver containing common air or oxygen gas, nothing but carbonic acid could be collected, nor was there any perceptible quantity of water condenſed againſt the ſides of the veſſel. That when 20 parts of this gas were mixed with eight parts of oxygen, and fired over mercury by the electric ſpark, the whole was reduced to 18 or 19 parts, which were found to be carbonic acid gas, being totally abſorbed by lime water. Hence we ſee, that this gas contains rather more oxygen than that produced from charcoal and finery cinder, as 20 parts of it, with eight of oxygen, produce no leſs than 18 or 19 parts of carbonic acid gas ; upon the whole, however, they appear to be very ſimilar, more particularly in their ſpecific gravities; but I remarked, nevertheleſs, that when a conſiderable quantity of the gaſeous oxide from charcoal, and the different calces was burned in a large receiver, ſome moiſture was depoſited on its ſides; there can be little doubt, therefore, that this difference in the reſults depends on a portion of hydrogen, which enters more or leſs into the compoſition of all common charcoal.

The ſtatement at page 7, taken from the foregoing and other experiments, will ſhew more correctly the difference between theſe gaſes, and ſome of the beſt known hydrocarbonates; from theſe calculations it will likewiſe appear, that the weight of the gaſeous oxide obtained from the decompoſition of the carbonic acid, added to that of the pure air neceſſary for its ſaturation, is very nearly equal to that of the carbonic acid gas produced; for 100 cubic inches of this gas weighs 30 grains, and 40 cubic inches of oxygen gas 13.6 grains, making together 43.6 grains. Now 92 cubic inches of carbonic acid gas weighs 43.2 grains; a greater degree of correctneſs cannot well be expected from experiments of this nature. But by making a ſimilar calculation with the products obtained from the exploſion of the gaſeous oxides from charcoal and the metallic calces, it will be found, that the weight of the gaſeous oxide, added to that of the pure air, is greater than the weight of the carbonic acid produced ; a circumſtance which would ſeem to prove the formation of water, more eſpecially when compared with the other experiments already related. From theſe facts, therefore, we would infer, that the pureſt gaſeous oxide is to be obtained from the decompoſition of carbonic acid; for in this caſe it appears to contain neither water, nor the baſis of water.

Dr. Prieſtley, in his obſervations on theſe gaſes remarks, that in their properties they are very ſimilar to the inflammable air obtained by heat from moiſtened charcoal; this, however, upon a more particular inveſtigation, I did not find to be the caſe. A quantity of

of moiſtened charcoal being introduced into a coated glaſs retort, heat was applied, and gradually increaſed until the retort became red. The firſt portions of gas collected conſiſted of nine parts of carbonic acid gas, and 57 hydrocarbonate; what came over about the middle of the proceſs was three parts of carbonic acid, with 55 hydrocarbonate, and the laſt portions were nearly pure hydrocarbonate, the whole amounting to ſeveral gallons.

Having ſeparated the carbonic acid from this gas by repeated agitation with lime water, I found that it was ſpecifically lighter than common air, in the proportion of 11 to 23; in this reſpect, therefore, it differs eſſentially from the gaſeous oxides, being not more than half their weight; I likewiſe found, that when burned in a receiver containing pure or common air, a very conſiderable quantity of water was produced, and condenſed againſt the ſides of the veſſel; the receiver likewiſe contained much carbonic acid gas. I next endeavoured to aſcertain the proportion of oxygenous gas neceſſary to ſaturate it. After ſeveral trials I found, when ſix meaſures of this gas well waſhed, were mixed with four of pure oxygen, and exploded over mercury by the electric ſpark, that the reſiduary gas amounted to $2\frac{1}{2}$ meaſures nearly, which was entirely carbonic acid. Here again we find a remarkable difference between this hydrocarbonate and the gaſeous oxides, for ſix meaſures of oxygenous gas produce only $2\frac{1}{2}$ of carbonic acid, when combined with the former; whereas with the latter, the ſame quantity would produce no leſs than 14 meaſures of the ſame acid gas. The production of water from the two gaſes is likewiſe very different, being little or none from the oxides, but very copious from the hydrocarbonate. Being deſirous to aſcertain if any of the known hydrocarbonates were ſimilar in their properties to the gaſeous oxides, I made comparative experiments with moſt of them, but there were none which appeared to contain oxygen, at leaſt in any quantity. The pureſt hydrocarbonates are obtained from camphor, by making its vapour paſs through a red hot tube; from ether by the ſame proceſs; from animal ſubſtances, and ſome vegetables, by deſtructive diſtillation; and where it was not expected, from the gas of marſhes, for I had always ſuppoſed that this gas was ſimilar to that obtained from moiſtened charcoal by diſtillation. All theſe pure hydrocarbonates have exactly the ſame properties; when well freed from carbonic acid gas, they are lighter than common air in the proportion of 15.5 to 23.5, or 2 to 3 nearly; two parts by meaſure require no leſs than $3\frac{1}{2}$ of pure oxygen to ſaturate them, the products being $2\frac{1}{4}$ parts carbonic acid with ſome water (ſee the table) One of their moſt remarkable properties however, is, that when mixed with $\frac{2}{3}$ their bulk of pure oxygen, or thereabout, and fired in a jar by the electric ſpark, inſtead of a diminution, there is a conſiderable increaſe of volume, notwithſtanding the production of carbonic acid. The following may be conſidered as the mean reſult of ſeveral experiments made in this way.

Six meaſures of hydrocarbonate, mixed with $4\frac{1}{2}$ of pure oxygen, were introduced into a ſtrong glaſs jar over mercury, being fired by the electric ſpark, the mixture after the exploſion, which was violent, was augmented to $12\frac{3}{4}$ meaſures, or there was an increaſe of $2\frac{1}{4}$ meaſures nearly. Lime water being admitted, it ſoon became turbid, and there was

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an abſorption of ſomewhat better than two meaſures. The remaining gas amounting to $10\frac{1}{2}$ meaſures, was found by the teſt of nitrous gas to contain no ſenſible quantity of diſengaged oxygen. Two meaſures of this reſiduary gas being fired with one of oxygen, were reduced to one meaſure, which was found to be carbonic acid gas; hence the whole reſiduary gas would require about $5\frac{1}{4}$ meaſures of oxygen to ſaturate it, and produce five meaſures of carbonic acid. It would appear, therefore, that notwithſtanding the apparent change which the hydrocarbonate underwent in the firſt exploſion, yet the final reſults are not ſenſibly varied, for by comparing the two experiments together, it will be found that the proportions of hydrocarbonate, oxygen, and carbonic acid produced, are very nearly as three to one, which they ſhould be where no change is effected. I varied theſe experiments in different ways, but conſtantly found the ſame reſults.

Alcohol may likewiſe be converted into a ſpecies of hydrocarbonate, by making its vapour paſs through a red hot tube; and it is rather ſingular that this gas ſhould be ſo different in its properties from that obtained from æther in the ſame way; for the latter, as has been already obſerved, is a pure hydrocarbonate.

Having obtained a large quantity of this hydrocarbonate from the decompoſition of alcohol, it was well agitated with lime water, but little or no diminution took place, a circumſtance which would ſeem to prove, that pure ſpirit does not contain oxygen. I next took its ſpecific gravity, and found that it was lighter than common air in the proportion of 12 to 23, whereas that from æther is lighter only in the proportion of 15 to 23. When burned in a receiver containing pure or common air, much carbonic acid, and a conſiderable proportion of water, were produced. After ſeveral trials I found, that four parts of this gas require $4\frac{2}{7}$ of oxygen to ſaturate them, the products being three parts of carbonic acid, with water. But four parts of hydrocarbonate from æther require ſeven of oxygen to ſaturate them, and $4\frac{1}{2}$ meaſures of carbonic acid with water are produced. Hence the difference between theſe gaſes muſt be manifeſt; it likewiſe appears from theſe experiments, that æther contains leſs carbone and more hydrogen than alcohol; for it will be found by calculation, that the proportion of carbone to hydrogen in æther is very nearly as 3 to 1, but in alcohol as 4 to 1; indeed this circumſtance ſeems to be confirmed by the depoſition of charcoal which takes place in the proceſs for obtaining æther.

From the foregoing experiments, therefore, we perceive, that none of the hydrocarbonates at preſent known, are ſimilar in their properties to the gaſeous oxides of carbone, being much lighter, and yielding a far leſs proportion of carbonic acid when combined with a given quantity of oxygen.

After acquiring a knowledge of the nature and properties of theſe gaſes, we are no longer at a loſs to account for their production; we know that the common hydrocarbonates conſiſt of carbone united with, or held in ſolution by hydrogen, ſo there can be little doubt that the gaſeous oxides are nothing more than carbone united with, or held in ſolution by oxygen, and then converted into the gaſeous ſtate by caloric; that they contain oxygen, is manifeſt from the very ſmall additional quantity which is found ſufficient

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to convert them into pure carbonic acid, from their being procured by a mixture of metallic calces and charcoal in the drieſt poſſible ſtate, in which proceſs the metallic calx is found to be revived, having parted with its oxygen to form the gaſeous oxide and acid; and from their being obtained by the partial decompoſition of this ſame acid, as in the experiments with chalk and iron filings, &c. From this laſt circumſtance it would alſo appear, that carbonic acid when raiſed to a high temperature may be decompoſed by any fixed ſubſtance which has a conſiderable affinity for oxygen. Upon the whole, therefore, we conſider the gaſeous oxide of carbone as holding the ſame relation to pure hydrocarbonate and carbonic acid; which nitrous gas, or the gaſeous oxide of azote, does to pure azote and nitrous acid. We likewiſe conceive, that the objections to the new ſyſtem of chemiſtry advanced on this head, by Dr. Prieſtley, are ſufficiently anſwered. We find that theſe gaſeous oxides are very different from what the Doctor conceived them to be, and that the preſence of water does not appear to be neceſſary to their production. We have no more reaſon therefore to ſuppoſe that the grey oxide of iron ſhould contain water, than that the other oxides, which give the very ſame products when mixed with charcoal and diſtilled, ſhould contain it likewiſe.

A Table ſhewing the Analyſis, &c. of the different Species of Hydrocarbonates, and the gaſeous Oxide of Carbone.

Gaſes, and the ſubſtances from which the gaſes are obtained, &c. &c.	Weight of 100 cubic inches or meaſures	Proportion of oxygen neceſſary to ſaturate 100 meaſures of the gas		Products when combined with oxygen				Hence the gaſes conſiſt of			
				Carbonic acid		Water produc.	Water held in ſolution by the gas	Oxygen	Carbone	Hydrogen	Water
				in volume	in quant.						
	Grains	Meaſures	Quantity Grains	Meaſures	Grains	Grains	Grains				
Pure hydrocarbonate from camphor, &c.	21.	176.	59.8	116.	54.5	18.	8. or 9.	none	11.	2+	8. or 9.
Ditto from æther	20.	170.	58.	108.	50.5	18.	9.	none	9.	3.	8.
Hydrogen from Alcohol	16.	118.	40.	75.	36.	13.	7.	none	7.	1.9	7.
Ditto from wet charcoal	14.5	66.	22.4	40.	19.	9.	9.	none	4 nearly	1.3	9.
Gaſeous oxides from charcoal and metallic oxides	30.	44.	15.	76.	35.5	about 8.	probably none	about 15.	7.	1+	uncertain
Gaſeous oxides from iron filings and carbonate of lime, or barytes.	30.	40.	13.6	92.	43.2	none	none	21+	8.6	none	none

In the preceding table, the weight of one 100 cubic inches of common air, under the mean preſſure of the atmoſphere, and at the temperature of 55° feet, is eſtimated at 31 grains; the quantity of pure carbone in carbonic acid at $\frac{1}{5}$ of the whole nearly, and the proportion of oxygen to hydrogen in water as 85 to 15.

The quantity of oxygen in the gaſeous oxides was thus eſtimated. In the laſt line, for example, we find that 30 grains of the oxide, produced, when combined with 13.6 grains of

of oxygen, 43.3 grains of carbonic acid. Now this acid consists of 8.6 grains of carbone, and 34.6 of oxygen nearly; but the quantity of oxygen employed was only 13.6 grains, hence the difference between 34.6 and 13.6, or 21 grains, must have been originally contained in the gas. It would therefore appear, that the gaseous oxides in their purest state consist of oxygen and carbone in the proportion of 21 to 9 nearly, combined with caloric.

Woolwich, March 19, 1801.

II.

On the Power of penetrating into Space by Telescopes; with a comparative Determination of the Extent of that Power in natural Vision, and in Telescopes of various Sizes and Constructions; illustrated by select Observations. By WILLIAM HERSCHEL, LL. D. F. R. S.

(*Concluded from Vol. IV. page 554.*)

JULY 12, 1784. I viewed the same nebula with a large 20-feet Newtonian reflector; power 157.

" A most beautiful extensive cluster of stars, of various magnitudes, very compressed in " the middle, and about 8′ in diameter, besides the scattered ones, which do more than " fill the extent of the field of view *: the large stars are red; the small ones are pale red. " RA $18^h\ 23'\ 39''$; PD 114° 7′."

The penetrating power of the first instrument was 39, that of the latter 61; but, from the observations, it is plain how much superior the effect of the latter was to that of the former, notwithstanding the magnifying power was so much in favour of the instrument with the small penetrating power.

July 30, 1783. With a small 20-feet Newtonian reflector, I viewed the nebula in the hand of Serpentarius, discovered by Mr. Messier, in 1764.

" With a power of 200, I see it consists of stars. They are better visible with 300. " With 600, they are too obscure to be distinguished, though the appearance of stars is still " preserved. This seems to be one of the most difficult objects to be resolved. With me, " there is not a doubt remaining; but another person, in order to form a judgment, ought " previously to go through all the several gradations of nebulæ which I have resolved into " stars."

May 25, 1791. I viewed the same nebula with a 20-feet reflector of my construction, having a penetrating power of $\frac{\sqrt{,64 \times \overline{188}^2}}{2} = 75{,}08$.

* This field, by the passage of an equatorial star, was 15′ 3″.

"With a magnifying power of 157, it appears extremely bright, round, and eaſily re-
"ſolvable. With 300, I can ſee the ſtars. It reſembled the cluſter of ſtars taken at 16^h
"$43' 40''$ *, which probably would put on the ſame appearance as this, if it were at a diſ-
"tance half as far again as it is. RA $17^h 26' 19''$; PD 93° 10′."

Here we may compare two obſervations; one taken with the penetrating power of 39, the other with 75; and, although the former inſtrument had far the advantage in magnifying power, the latter certainly gave a more complete view of the object.

The 20-feet reflector having been changed from the Newtonian form to my preſent one, I had a very ſtriking inſtance of the great advantage of the increaſed penetrating power, in the diſcovery of the Georgian ſatellites. The improvement, by laying aſide the ſmall mirror, was from 61 to 75: and, whereas the former was not ſufficient to reach theſe faint objects, the latter ſhewed them perfectly well.

March 14, 1798. I viewed the Georgian planet with a new 25-feet reflector. Its penetrating power is $\frac{\sqrt{,64 \times 240^2}}{2} = 95,85$; and, having juſt before alſo viewed it with my 20-feet inſtrument, I found, that with an equal magnifying power of 300, the 25-feet teleſcope had conſiderably the advantage of the former.

Feb. 24, 1786. I viewed the nebula near Flamſteed's 5th Serpentis, which has been mentioned before, with my 20-feet reflector; magnifying power 157.

"The moſt beautiful extremely compreſſed cluſter of ſmall ſtars; the greateſt part of
"them gathered together into one brilliant nucleus, evidently conſiſting of ſtars, ſur-
"rounded with many detached gathering ſtars of the ſame ſize and colour. RA $15^h 7'$
"$12''$; PD 87° 8′."

May 27, 1791. I viewed the ſame object with my 40-feet teleſcope; penetrating power $\frac{\sqrt{,64 \times 480^2}}{2} = 191,69$; magnifying power 370.

"A beautiful cluſter of ſtars. I counted about 200 of them. The middle of it is ſo
"compreſſed that it is impoſſible to diſtinguiſh the ſtars."

Here it appears, that the ſuperior penetrating power of the 40-feet teleſcope enabled me even to count the ſtars of this nebula. It is alſo to be noticed, that the object did not ſtrike me as uncommonly beautiful; becauſe, with much more than double the penetrating, and alſo more than double the magnifying power, the ſtars could not appear ſo compreſſed and ſmall as in the 20-feet inſtrument: this, very naturally, muſt give it more the reſemblance of a coarſer cluſter of ſtars, ſuch as I had been in the habit of ſeeing frequently.

* The object referred to is No. 10. of the *Connoiſſance des Temps* for 1783, called "*Nebuleuſe ſans*
"*etoiles.*" My deſcription of it is, "A very beautiful, and extremely compreſſed, cluſter of ſtars: the
"moſt compreſſed part about 3 or 4′ in diameter. RA $16^h 46' 2''$; PD 93° 46′."

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The 40-feet telescope was originally intended to have been of the Newtonian construction; but, in the year 1787, when I was experimentally assured of the vast importance of a power to penetrate into space, I laid aside the work of the small mirror, which was then in hand, and completed the instrument in its present form.

" Oct. 10, 1791. I saw the 4th satellite and the ring of Saturn, in the 40-feet speculum, without an eye-glass."

The magnifying power on that occasion could not exceed 60 or 70; but the great penetrating power made full amends for the lowness of the former; notwithstanding the greatest part of it must have been lost for want of a greater opening of the iris, which could not take in the whole pencil of rays, for this could not be less than 7 or 8 tenths of an inch.

Among other instances of the superior effects of penetration into space, I should mention the discovery of an additional 6th satellite of Saturn, on the 28th of August, 1789; and of a 7th, on the 11th of September, in the same year; which were first pointed out by this instrument. It is true that both satellites are within the reach of the 20-feet telescope: but it should be remembered, that when an object is once discovered by a superior power, an inferior one will suffice to see it afterwards. I need not add, that neither the 7 nor 10-feet telescopes will reach them; their powers, 20 and 29, are not sufficient to penetrate to such distant objects, when the brightness of them is not more than that of these satellites. It is also evident, that the failure in these latter instruments, arises not from want of magnifying power; as either of them has much more than sufficient for the purpose.

Nov. 5, 1791. I viewed Saturn with the 20 and 40-feet telescopes.

" 20-feet. The 5th satellite of Saturn is very small. The 1st, 2d, 3d, 4th, 5th, and the new 6th satellite, are in their calculated places."

" 40-feet. I see the new 6th satellite much better with this instrument than with the 20-feet. The 5th is also much larger here than in the 20-feet; in which it was nearly the same size as a small fixed star, but here it is considerably larger than that star."

Here the superior penetrating power of the 40-feet telescope shewed itself on the 6th satellite of Saturn, which is a very faint object; as it had also a considerable advantage in magnifying power, the disk of the 5th satellite appeared larger than in the 20-feet. But the small star, which may be said to be beyond the reach of magnifying power, could only profit by the superiority of the other power.

Nov. 21, 1791. 40-feet reflector; power 370.

" The black division upon the ring is as dark as the heavens about Saturn, and of the same colour."

" The shadow of the body of Saturn is visible upon the ring, on the following side; its colour is very different from that of the dark division. The 5th satellite is less than the 3d; it is even less than the 2d."

20-feet reflector; power 300.

" The 3d fatellite feems to be fmaller than it was the laft night but one. The 4th fa-
" tellite feems to be larger than it was the 19th. This telefcope fhews the fatellites not
" nearly fo well as the 40-feet.

Here, the magnifying power being nearly alike, the fuperiority of the 40-feet telefcope muft be afcribed to its penetrating power.

The different nature of the two powers above mentioned being thus evidently eftablifhed, I muft now remark, that, in fome refpects, they even interfere with each other; a few inftances of which I fhall give.

Auguft 24, 1783. I viewed the nebula north preceding Flamfteed's 1 Trianguli, difcovered by Mr. Meffier, in 1764.

" 7-feet reflector; power 57. There is a fufpicion that the nebula confifts of exceed-
" ingly fmall ftars. With this low power it has a nebulous appearance; and it vanifhes
" when I put on the higher magnifying powers of 278 and 460."

Oct. 28, 1794. I viewed the fame nebula with a 7-feet reflector.

" It is large, but very faint. With 120, it feems to be compofed of ftars, and I think I
" fee feveral of them; but it will bear no magnifying power."

In this experiment, magnifying power was evidently injurious to penetrating power. I do not account for this upon the principle that by magnifying we make an object lefs bright; for, when opticians have alfo demonftrated that brightnefs is diminifhed by magnifying, it muft again be underftood as relating only to the *intrinfic* brightnefs of the magnified picture; its abfolute brightnefs, which is the only one that concerns us at prefent, muft always remain the fame *. The real explanation of the fact, I take to be, that while the light collected is employed in magnifying the object, it cannot be exerted in giving penetrating power.

June 18, 1799. I viewed the planet Venus with a 10-feet reflector.

" Its light is fo vivid that it does not require, nor will it bear, a penetrating power of
" 29, neither with a low nor with a high magnifying power."

* This may be proved thus. The mean intrinfic brightnefs, or rather illumination, of a point of the picture on the retina, will be *all the light that falls on the picture, divided by the number of its points*; or $C = \frac{l}{N}$. Now, fince with a greater magnifying power m, the number of points N increafes as the fquares of the power, the expreffion for the intrinfic brightnefs $\frac{l}{N}$, will decreafe in the fame ratio; and it will confequently be in general $N \propto m^2$, and $\frac{l}{N}$ or $C \propto \frac{1}{m^2}$; that is, by compounding $CN \propto \frac{m^2}{m^2} = l = 1$; or abfolute brightnefs a given quantity. M. Bouguer has carefully diftinguifhed intrinfic and abfolute brightnefs, when he fpeaks of the quantity of light reflected from a wall, at different diftances. *Traité d'Optique*, page 39 and 40.

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This is not owing to the least imperfection in the mirror, which is truly parabolical, and shews, with all its aperture open, and a magnifying power of 600, the double star γ Leonis in the greatest perfection.

"It shewed Venus, perfectly well defined, with a penetrating power as low as 14, and "a magnifying power of 400, or 600."

Here, penetrating power was injurious to magnifying power; and that it necessarily must be so, when carried to a high pitch, is evident; for, by enlarging the aperture of the telescope, we increase the evil that attends magnifying, which is, that we cannot magnify the object without magnifying the medium. Now, since the air is very seldom of so homogeneous a disposition as to admit to be magnified highly, it follows that we must meet with impurities and obstructions, in proportion to its quantity. But the contents of the columns of air through which we look at the heavens by telescopes, being of equal lengths, must be as their bases, that is, as the squares of the apertures of the telescopes; and this is in a much higher ratio than that of the increase of the power of penetrating into space. From my long experience in these matters, I am led to apprehend, that the highest power of magnifying may possibly not exceed the reach of a 20 or 25-feet telescope; or may even lie in a less compass than either. However, in beautiful nights, when the outside of our telescopes is dropping with moisture discharged from the atmosphere, there are now and then favourable hours, in which it is hardly possible to put a limit to magnifying power. But such valuable opportunities are extremely scarce; and, with large instruments, it will always be lost labour to observe at other times.

As I have hinted at the natural limits of magnifying power, I shall venture also to extend my surmises to those of penetrating power. There seems to be room for a considerable increase in this branch of the telescope; and, as the penetrating power of my 40-feet reflector already goes to 191,69, there can hardly be any doubt but that it might be carried to 500, and probably not much farther. The natural limit seems to be an equation between the faintest star that can be made visible, by any means, and the united brilliancy of star-light. For, as the light of the heavens, in clear nights, is already very considerable in my large telescope, it must in the end be so increased, by enlarging the penetrating power, as to become a balance to the light of all objects that are so remote as not to exceed in brightness the general light of the heavens. Now, if P be put for penetrating power, we have $\sqrt{\frac{P^2 a^2}{x}} = A =$ 10 feet 5,2 inches for an aperture of a reflector, on my construction, that would have such a power of 500.

But, to return to our subject; from what has been said before, we may conclude, that objects are viewed in their greatest perfection, when, in penetrating space, the magnifying power is so low as only to be sufficient to shew the object well; and when, in magnifying objects, by way of examining them minutely, the space-penetrating power is no higher than what will suffice for the purpose; for, in the use of either power, the injudicious overcharge of the other, will prove hurtful to perfect vision.

It

It is remarkable that, from very different principles, I have formerly determined the length of the visual ray of my 20-feet telescope upon the stars of the milky way, so as to agree nearly with the calculations that have been given *. The extent of what I then figuratively called my sounding line, and what now appears to answer to the power of penetrating into space, was shewn to be not less than 415, 461, and 497 times the distance of Sirius from the sun. We now have calculated that my telescope, in the Newtonian form, at the time when the paper on the Construction of the Heavens was written, possessed a power of penetration, which exceeded that of natural vision 61,18 times; and, as we have also shewn, that stars at 8, 9, or at most 10 times the distance of Sirius, must become invisible to the eye, we may safely conclude, that no single star, above 489,551, or at most 612 times as far as Sirius, can any longer be seen in this telescope. Now, the greatest length of the former visual ray, 497, agrees nearly with the lowest of these present numbers, 489; and the higher ones are all in favour of the former computation; for that ray, though taken from what was perhaps not far from its greatest extent, might possibly have reached to some distance beyond the apparent bounds of the milky way: but, if there had been any considerable difference in these determinations, we should remember that some of the data by which I have now calculated are only assumed. For instance, if the opening of the iris, when we look at a star of the 7th magnitude, should be only one-tenth of an inch and a half, instead of two, then *a*, in our formula, will be = 1,5; which, when resolved, will give a penetrating power of 81,58; and therefore, on this supposition, our telescope would easily have shewn stars 571 times as far from us as Sirius; and only those at 653,734, or 816 times the same distance, would have been beyond its reach. My reason for fixing upon two-tenths, rather than a lower quantity, was, that I might not run a risk of over-rating the powers of my instruments. I have it however in contemplation, to determine this quantity experimentally, and perceive already, that the difficulties which attend this subject may be overcome.

It now only remains to shew, how far the penetrating power, 192, of my large reflector, will really reach into space. Then, since this number has been calculated to be in proportion to the standard of natural vision, it follows, that if we admit a star of the 7th magnitude to be visible to the unassisted eye, this telescope will shew stars of the one thousand three hundred and forty-second magnitude.

But, as we did not stop at the single stars above mentioned, when the penetration of the natural eye was to be ascertained, so we must now also call the united lustre of sidereal systems to our aid in stretching forwards into space. Suppose therefore, a cluster of 5000 stars to be at one of those immense distances to which only a 40-feet reflector can reach, and our formula will give us the means of calculating what that may be. For, putting *S*

* Phil. Trans. Vol. LXXV. page 247, 248.

for

for the number of stars in the cluster, and D for its distance, we have $\frac{\sqrt{n A^2 S}}{a} = D$*; which, on computation, comes out to be above $11\frac{3}{4}$ millions of millions of millions of miles! a number which exceeds the distance of the nearest fixed star, at least three hundred thousand times.

From the above considerations it follows, that the range for observing, with a telescope such as my 40-feet reflector, is indeed very extensive. We have the inside of a sphere to examine, the radius of which is the immense distance just now assigned to be within the reach of the penetration of our instruments, and of which all the celestial objects visible to the eye, put together, form as it were but the kernel, while all the immensity of its thick shell is reserved for the telescope.

It follows, in the next place, that much time must be required for going through so extensive a range. The method of examining the heavens, by sweeping over space, instead of looking merely at places that are known to contain objects, is the only one that can be useful for discoveries.

In order therefore to calculate how long a time it must take to sweep the heavens, as far as they are within the reach of my 40-feet telescope, charged with a magnifying power of 1000, I have had recourse to my journals, to find how many favourable hours we may annually hope for in this climate. It is to be noticed, that the nights must be very clear; the moon absent; no twilight; no haziness; no violent wind; and no sudden change of temperature; then also, short intervals for filling up broken sweeps will occasion delays; and, under all these circumstances, it appears that a year which will afford 90, or at most 100 hours, is to be called very productive.

In the equator, with my 20-feet telescope, I have swept over zones of two degrees, with a power of 157: but, an allowance of 10 minutes in polar distance must be made, for lapping the sweeps over one another where they join.

As the breadth of the zones may be increased towards the poles, the northern hemisphere may be swept in about 40 zones; to these we must add 19 southern zones; then, 59 zones, which, on account of the sweeps lapping over one another about 5′ of time in right ascension, we must reckon of 25 hours each, will give 1475 hours. And, allowing 100 hours per year, we find that, with the 20-feet telescope, the heavens may be swept in about 14 years and $\frac{3}{4}$.

Now, the time of sweeping with different magnifying powers will be as the squares of the powers; and, putting p and t for the power and time in the 20-feet telescope, and $P = 1000$ for the power in the 40, we shall have $p^2 : t :: P^2 : \frac{t P^2}{p^2} = 59840$. Then, making

* $D = 11765475948678678679$ miles.

king the ſame allowance of 100 hours per year, it appears that it will require not leſs than 598 years, to look with the 40-feet reflector, charged with the abovementioned power, only one ſingle moment into each part of ſpace; and, even then, ſo much of the ſouthern hemiſphere will remain unexplored, as will take up 213 years more to examine.

Slough, near Windſor, June 20, 1799.

III.

Enquiries relative to the Laws of Affinity. By CITIZEN BERTHOLLET, *Member of the National Inſtitute, &c. Abridged by* CIT. BOUILLON LAGRANGE.*

THE publication of the work of Cit. Berthollet on the affinities, has long been expected by chemiſts. Their anxiety was doubtleſs very natural; for an object ſo important could only be completed by that celebrated individual, whoſe diſcoveries have been ſo highly advantageous to ſociety.

The work here announced cannot be abridged without difficulty; for by preſenting the reader with a mere analyſis of each article, much information would be loſt. Hence we ſhall frequently ſuffer the author to ſpeak for himſelf, as his work is a collection of facts, upon which the theory is eſtabliſhed.

Cit. Berthollet has divided his memoir into fifteen articles:

In the firſt of theſe, the author makes a panegyric upon Bergmann, who devoted his attention to this ſubject with the greateſt ſucceſs. Cit. Berthollet has principally attended to the work of the Swediſh chemiſt, when treating on this ſubject. He then ſtates the object of his memoir: "It is my intention" ſays he "to prove that the elective affinities do not act like abſolute forces, by which one ſubſtance might be diſplaced by another in a combination; but that in all the compoſitions and decompoſitions which are owing to elective affinity, a diviſion is made of the object of the combination between the ſubſtances whoſe actions are oppoſed; and that the proportions of this diviſion are determined not only by the energies of affinity of theſe ſubſtances, but alſo by the quantity with which they act; ſo that the quantity may compenſate for a want of the affinity, and produce a ſimilar degree of ſaturation.

If, continues the author, I eſtabliſh that the quantity of a ſubſtance can ſupply the want of force of its affinity, it will follow that its action will be proportionate to the quantity neceſſary to produce a determinate degree of ſaturation. I denominate *maſs*, that quantity which is the *meaſure* of the capacity of the ſaturation of different ſubſtances.

When therefore I compare the affinities of ſubſtances, I ſhall direct my attention to the ponderable quantity, which in this compariſon ought to be equal; but, when I compare

* Annales de Chimie, XXXVI. 302.

their

their action, which is composed of both their affinity and their proportion, I must then consider the mass.

The author next observes, that in the following discussions, he shall principally make use of acids and alkalis, among which he comprises those earths which act as such; because they exercise great powers, which may dispel the influence of slight causes; and because they often produce comparable degrees of saturation, and afford results which may be easily observed. But the consequences drawn from their properties by Cit. Berthollet, are applicable to all other combinations. It is proved by many examples, that the principle established by the author extends to the whole chemical action of bodies.

After having proved by direct experiments, that the chemical action of substances, of which the forces are opposed, does not depend upon their affinity alone, but likewise on their quantity, the author states that he shall make choice of observations on different kinds of combinations which will confirm this principle, and prove its extent. I shall examine, he continues, the circumstances which modify it, or those affections of bodies which favour or diminish their chemical action, and which cause the proportions of the combinations which they are capable of forming to vary: I shall apply these considerations to the complex affinities, and to those of compound bodies; and lastly, I shall endeavour to determine the basis on which the general and particular theories of chemical phenomena ought to be established.

ARTICLE II.

Experiments which prove that in the Elective Affinities, the opposing Substances divide that Body which is the Subject of the Combination.

To demonstrate the truth of this proposition, Cit. Berthollet describes several experiments, from which we shall quote the following: I kept, says he, in a state of ebullition in a small quantity of water, an equal weight of pot-ash, purified by alcohol, and of sulphate of barytes. The operation was made in a retort, and consequently without the contact of air, and continued till the mixture was evaporated to dryness: the residuum was treated with alcohol, which dissolved the pot-ash, and afterwards with water. The latter effected a solution which also exhibited alkaline properties. This alkali was saturated with acetous acid, after which there was formed, by evaporation, a considerable quantity of small crystals, which possessed all the characters of sulphate of pot-ash; so that the sulphate of barytes was partly decomposed by the pot-ash, and the sulphuric acid was divided between the two bases.

The other experiments were made,

1. With sulphate of pot-ash and lime: 2. With oxalate of lime and pot-ash: 3. With oxalate of lime and nitric acid: 4. With phosphate of lime and pot-ash: 5. With pot-ash and carbonate of lime: 6. With soda and sulphate of pot-ash.

In the whole of these experiments it is seen that those bases, which are admitted as forming, with the acids, the strongest and most permanent combinations, were partly

extricated by a basis to which a weaker affinity is attributed; so that the acid was divided between the two bases. It is likewise observed that the acids are partly disengaged from their base by others, the affinity of which is considered as inferior, so that the base is divided between two acids.

The author premises, that if a small quantity only of the decomposing substance be employed, the effect will not be perceptible, and he concludes, that when any substance acts upon a combination, that which is the subject of the combination is divided between the two other substances, not only according to the respective energies of their affinity, but likewise according to their quantity.

ARTICLE III.

Observations which confirm the Principle of the Chemical Action in Proportion to the Mass.

The author makes observations on different kinds of combination exposed to an elective affinity, and examines whether the principle of chemical action, in proportion to the mass, be not precisely applicable to their explanation.

If, as he observes, we treat the carbonate of pot-ash with lime, we cannot deprive the pot-ash of all its carbonic acid, even by making successive operations with fresh lime; and if we evaporate the fluid, the residuum is still capable of effervescence, on being saturated by the acids, because the pot-ash which remained present with the lime opposed its action; and, the greater the quantity of carbonic acid which has been before absorbed by the lime, the more powerful is the pot-ash to maintain its combination with this acid.

When the equilibrium is established between the action of the lime and the resistance of the pot-ash, if the liquid be filtered and evaporated, we may take away, by a weak affinity, that portion of alkali which is superabundant to the constitution of carbonate of pot-ash, that is to say, the whole portion which is not defended by a considerable mass of carbonic acid. Alcohol possesses this property; the separation is effected by this fluid. The carbonate of pot-ash remains in solution in a small quantity of water, while the alcohol of pot-ash floats above. The separated carbonate may again be treated with lime; and by this second operation it may be reduced to an inconsiderable quantity.

Cit. Berthollet also quotes several other experiments relative to the chemical action in proportion to the mass.

If a carbonate which contains an excess of pot-ash be treated with alcohol, it can only be deprived of a part of its excess by such treatment.

The other neutral salts likewise possess the property of retaining a part of that pot-ash which may be contained in them superabundant to their neutralization.

It is also known that phosphate of lime cannot be entirely decomposed by the sulphuric acid; though it has been asserted that the sulphuric acid has a greater affinity to lime than the phosphoric acid.

The

The ſame happens when the ſulphate of alumine is decompoſed by ammonia: the precipitate always contains a portion of ſulphuric acid.

If the magneſia of the ſulphate of alumine (qu. magneſia?) be precipitated by pot-aſh, the magneſia will alſo contain a portion of ſulphuric acid; for when the magneſia is ſubmitted to ſtrong heat, it is afterwards found to poſſeſs rather a ſtrong taſte of ſulphate.

All theſe experiments prove, that in chemical analyſes we are liable to error, by taking that for the real weight, of alumine or magneſia, which may have been contained in a compound ſubſtance, and is merely the precipitate formed by elective affinity.

From the preceding obſervations, and many others which cannot here be given, it appears that in operations of elective affinity the ſubject of the combination is divided between the two ſubſtances which act upon it, in conſequence of the forces they are capable of oppoſing to each other.

A circumſtance worthy of attention, and which clearly proves that the chemical action depends as much on the quantity as on the affinity of ſubſtances, is, that to produce oppoſite effects, it is only neceſſary to make the requiſite alteration in the quantities.

ARTICLE IV.

On thoſe Modifications of Chemical Action, which ariſe from the Inſolubility of the Subſtances.

The author ſucceſſively examines thoſe affections of bodies which may diſguiſe or modify the applications of the principle eſtabliſhed in the preceding articles. He proves by a number of experiments; 1. The manner in which an inſoluble ſubſtance acts when oppoſed to a combination. 2. If a ſubſtance poſſeſſes ſome degree of ſolubility, its action is compoſed of that of the diſſolved part, and of that which remains ſolid. 3. If an inſoluble combination be attacked by a liquid ſubſtance, the inconveniencies of its inſolubility ſpeedily diſappear, in the caſe when it is only neceſſary that the inſoluble ſubſtance ſhould loſe a part of its conſtituent principles, to become liquid.

The author next proceeds to examine the difference of ſpecific gravities of the inſoluble ſubſtance, and the liquid. According to him, this has an influence on their reſpective actions, even when agitation and heat are employed; becauſe it continually tends to ſeparate the inſoluble ſubſtance, and to withdraw from the action oppoſed to it. Hence there is a difference on this account between the ſulphates of barytes and of alumine.

Laſtly, as the inſolubility prevents thoſe proportions of diviſion from taking place, which might elſe have been eſtabliſhed by the oppoſing forces, ſo likewiſe it is found to retard the operation of thoſe which can take place; ſo that it may eaſily produce deception by the appearances which it affords at the commencement of an operation. For example, when we mix ſome concentrated ſulphuric acid with a ſolution of ſulphate of pot-aſh, or any other ſalt which requires a large proportion of water to diſſolve it, the acid ſoon combines with the water; and the ſalt which loſes its liquidity is precipitated; but by continuing the experiment, and multiplying the contacts, the ſalt becomes diſſolved, and enters into combination with the liquid.

ARTICLE V.

On Cohesion and Crystallization.

The cohesion of the particles of a body is owing to the mutual affinity of those particles: it is a force which must be overcome by the action of the substance which has a tendency to combine with those parts, or to decompose their combination. It is known that clay, the particles of which have formed a strong cohesion by desiccation, is no longer attacked by an acid which would be capable of dissolving it when in another state.

The mutual affinity of the saline particles is the cause of crystallization; and this modifies the chemical action in a manner which deserves attention. The author makes some remarks on this subject, as well as on the force which produces crystallization in a saline solution. It establishes a limit to which the degree of saturation of the salt in water can arrive; so that if the water does not dissolve a greater quantity of it, this is not in consequence of its affinity being satisfied, but because it is no longer sufficiently powerful to overcome the resistance of the crystallization. From these and numerous other considerations, Cit. Berthollet concludes that the force of cohesion, which has hitherto been considered merely as an obstacle to solution, determines the quantities of substances which may be brought into action in a liquid; and thus modifies the conditions of chemical action. It is this also which causes those separations that take place, either by crystallization or precipitation, and determines the proportions of the combinations which are formed by separation from the liquid, when the solubility or insolubility depends upon these proportions.

ARTICLE VI.

On the Elasticity of Substances which exert a Chemical Action.

When any substance escapes in a gaseous state, in proportion as it is disengaged from an intimate combination, the whole of that part which assumes the elastic state, ceases to afford any resistance; so that the substance no longer acts in the ratio of its mass. The substance presented may in this case render the decomposition complete; and it will be sufficient to use simply that quantity which would have been requisite for immediately forming the combination in which it is to enter, or at least a small excess only will be necessary.

This happens, as Cit. Berthollet observes, with the carbonic acid, when it forms a carbonate, and another acid is presented to it. This last acid, which acts in proportion to its mass, may, even with an affinity inferior to that of the carbonic acid, successively expel it from the combination, even till none shall remain, provided it be employed at first in a quantity rather greater than would have been necessary for immediately forming its combination with the basis.

From all these facts presented by the author, it follows that elasticity produces effects analogous to those of the force of cohesion, by modifying, in an opposite direction, the effects of the affinity proper to each substance.

 ARTICLE

ARTICLE VII.

Of the Action of Caloric.

In this chapter Cit. Berthollet examines the action of caloric upon bodies, and the phenomena it produces in the various combinations or decompositions. Articles VIII. and IX. contain a great number of experiments on efflorescence and on solvents.

What Cit. Berthollet understands by efflorescence, is the property of rising above the mass, and of separating from it by chemical action.

In order that soda may rise in efflorescence, he observes, it must necessarily have combined with carbonic acid, which it can attract from the air of the atmosphere; but the action of the carbonic acid, which is very small in quantity, and in an elastic state, cannot sensibly add to the force which has produced the separation of the soda, from the combination in which it is found; it only separates the eliminated part, and hinders it from continuing to act upon the combination.

With respect to solvents, the author is of opinion that their use is to overcome the resistance which arises from the cohesion of the parts intended to be brought into action, or from their elasticity, and to increase their mutual contact.

Solvents act upon substances, which they dissolve by their affinity and their quantity, like all substances which have a tendency to combination; and to these is applicable all the doctrine of the combination of bodies. The author takes water for his example, which is most frequently employed as a solvent.

Cit. Berthollet then examines the circumstances in which this action may change or sensibly modify the results; and this leads him to offer a series of experiments; on which he establishes a luminous theory.

This article is concluded by some remarks upon caloric. The author conceives it to act upon bodies which are not equally dilatable in a manner similar to solvents, by attacking the force of cohesion, and thus enabling the particles to exercise their mutual action. Its action concurs with those of the solvents, in opposing the force of cohesion: whence it happens, that the solution of a salt by water varies according to the degrees of temperature.

When caloric acts upon substances not equally dilatable, it produces separations and new combinations, independent of the affinity peculiar to substances; in the same manner as the solvents when they act upon substances not equally soluble.

(To be continued.)

Supplementary

IV.

Supplementary Letter on the Syphon, to the Communication at page 547 *of the last Volume. By Mr.* William Close.

To Mr. NICHOLSON.

SIR,

VALVES covered with leather may be adapted to the ends of the ſmall pipes, and they may be opened by drawing a wire after the external valve has cloſed its tube. For this purpoſe a long ſmall pipe muſt be fixed into the bottom of A W, (Pl. XXIV. Fig. 1.) muſt riſe to near the top of that veſſel, and its lower end be immerſed in a veſſel of water below the loweſt part of the ſyphon. The wire muſt paſs through this pipe, and be fixed to one lever within the veſſel for raiſing the valves, and to another below the bottom of the pipe, connected with the moveable veſſels above. When the ſecond and fourth veſſels aſcend, the valves will open, unleſs they are prevented by the weight of water in the ſyphon, and water will riſe in the ſmall wire pipe, but cannot enter the veſſel A W. The aſcent, however, will extend through a ſpace nearly equal to the length of the deſcending column in the ſyphon.

A very eaſy and extenſive method of rendering active the preſſure of the atmoſphere is here preſented to our notice. The veſſel A W may be filled with water through a ſmall vertical pipe, without receiving any from the ſyphon, to which it need only be connected by an air pipe.

By the ſame principle water may be raiſed to a conſiderable height above the ſyphon. To return, however, to our former ſubject, the water in the pipe will completely prevent the admiſſion of air into the receptacle of raiſed water: it ſerves the purpoſe of packing or leathering.

The water in the veſſel A W, will never reach the higher end of this pipe, becauſe it cannot riſe above the air pipe connected with the ſyphon.

If ſuch a long pipe ſhould be inconvenient, a much ſhorter one may be uſed, if its lower end be immerſed in mercury inſtead of water; it may then alſo be conſiderably wider than the other, and an iron rod of a rather leſs diameter may be placed within it to puſh up the valves. If the valve of the water tube be made light, it will open and ſhut without any aſſiſtance.

The veſſel A W. ſhould not be large; the top part of it ſhould be fixed on with ſcrews, to allow the valves to be eaſily taken out and repaired. Two cocks in the ſmall pipes oppoſite A and W will then be uſeful to prevent the depreſſion of the columns in the branches of the ſyphon.

It

It will be always adviseable to make the length of the descending branch of the syphon exceed that of the ascending one, as much as circumstances will admit, and to let the lowest part of it be made of a conical divergent form: the velocity of the current will be thus encreased, the vessel will be sooner filled, and the depression of the columns will be less liable to happen from very slight imperfections in the valves.

I am, SIR,

Your humble servant,

Dalton, Feb. 21, 1801. WILLIAM CLOSE.

CORRECTION.

There was a slight omission made in one paragraph of the letter before this, which may easily be corrected. Please to look for the following words at the beginning, page 547.

If the connexion between the vessel and syphon was made by two tubes *inserted into the shorter branch one immediately above the other*, the end of the *lower* turned against the current, and the end of the *higher* turned from it, the stream, &c. Insert the words with the line under them.

V.

On the Doctrine of Heat; particularly with regard to the States of dense and elastic Fluidity in Bodies. In a Letter from Mr. JOSEPH ASTLEY.

To MR. NICHOLSON.

SIR,

THAT the solid, liquid, and aeriform modifications of bodies depend on nothing more than the different proportions of caloric with which they are directly combined, is an opinion so attractive, from its apparent simplicity, and so closely associated with our most elementary notions respecting the physical laws of nature, that a degree of prejudice may perhaps be excited in many by the mere suggestion of a doubt upon the subject. Those intimate with chemical theories have learned, however, that the apparent simplicity of a doctrine is as frequently the result of an imperfect as of intire comprehension of the subject which it regards, and that our solicitude to establish the conformity of particular phenomena too often leads us to admit inconsistencies into our general principles of physical economy. The new lights which have been lately thrown on several of the phenomena in which evaporation is concerned, particularly on those of meteorology, have been sufficient to

create suspicion in the minds of many who have considered them attentively, that the doctrine which affirms this process to consist merely in a direct combination of caloric is not to be admitted without limitations. They have regarded these limitations, however, as only partial exceptions to a general law: and my present purpose is to show that, even in its most general acceptation, the doctrine is open to very weighty objections.

I shall endeavour to explain myself in examining what grounds we have for the supposed analogy between vaporization and liquefaction; what indications we have as to the real action of caloric in vaporization; and whether the probability both of analogy and of facts should not incline us rather to refer the vaporous modification of bodies to the class of chemical decompositions.

The most important and characteristic circumstance attending the phenomenon of liquefaction is the uniformity of temperature at which it takes place. And it is here that a most obvious failure in the supposed analogy of vaporization presents itself. It is not sufficient for this analogy that in similar circumstances water, like ice, cannot preserve its form beyond a certain temperature. It would require also that water, like ice, should not be capable of changing its form below that temperature. So little do we find this the case, that many circumstances connected with evaporation give us reason to conclude that, when not impeded by mechanical resistance, there is scarcely any temperature at which it may not take place; and that, without any variation but as to the rapidity with which it is performed. I shall enter into no examination here of the attempt to rescue the analogy in question from this objection by referring what is called the spontaneous evaporation of liquids to the solvent action of air, because in as much as respects the principle of the objection, the phenomenon is equally evident whether air be present or not.

But to come more directly to the point, let us attend to the circumstance by which supposed analogy between liquefaction and evaporation is deemed peculiarly expressed. This is the appearance in each case of a quantity of combined caloric. It will be seen in the sequel that I am far from being disposed to deny this combination in the phenomena of evaporation. All that I have to object to is the idea of its indicating a correspondence of principle between these phenomena and those of liquefaction. The appearance from which the combination of caloric in the former cases is inferred, are those of its becoming latent in the production, and again sensible in the condensation of vapour. It must be observed, however, in the first place, that the caloric concerned in these two phenomena is by no means the same. Mr. Watt found that vapour produced in vacuo at a temperature 70°, gave out, on its condensation, a quantity of caloric equal to 1200° or 1300°. And, as it must be needless to show that such a quantity could not have existed in the vapour on its original production, it is evident that this at least cannot be regarded as having any essential relation to its constitution.

Let us see how far the caloric which is actually present, and becomes latent in the formation of vapour, can be understood to denote a conformity between this modification and that of liquefaction. That this is not implied by the mere circumstance itself, is evident

evident from the variety of cases in which we see it to take place during chemical changes of the most different kinds, and even in some modifications which must be accounted purely mechanical. Before, therefore, the latent combination of caloric can serve as any index of physical analogy between two phenomena, there must be shown to exist a correspondence in the other changes with which it is respectively associated. So little do we find of this correspondence here, however, that the changes most intimately and essentially connected with it are in the one case an expansion, and in the other a diminution of bulk.

Nor do we find the comparison at all better supported, if, attributing the latent absorption of caloric to a previous change of capacity, we attempt to substitute this as an indication of the analogy in question*. This change of capacity itself requires, no less than the absorption of caloric, the supposition of a previous modification, and if, with the generality of authors, we refer this modification to change of form, the same difficulty occurs as before, viz. that of conceiving how a similar change of form can be attended with augmentation of volume in one case, and diminution of it in the other. Besides that to ascribe the change of capacity in vaporization and liquefaction to change of form, instead of bringing us to the cause of the phenomena, is only leading us back to the phenomena themselves, and thus returning to the same point from which we set out. The only way by which it has been attempted to escape from this circle has been by resting the immediate occasion of the phenomena on the change of temperature: and how very little this is to be trusted to as an index of analogy between vaporization and liquefaction, has already been seen.

But we may easily satisfy ourselves of the fallacy of the supposed analogy between these two modifications of any body, if we attempt to infer from it the correspondence of the changes which such a body should exhibit with respect to both in consequence of the same changes in the constitution. Thus, if the analogy were to be depended on, the same change of constitution by which the freezing point of water becomes lowered, should lower also its boiling point. I need not point out how grossly such inferences would mislead us. And if the solvent action of the air may serve to screen this analogy from the absurdity of some of these implications, it has at the same time introduced other inconsistencies which are equally gross. According to this, the influence of air and of caloric in producing evaporation being similar, we should expect that when the action of the former is added to that of the latter, the evaporation should be more speedy than when the latter exerts itself alone. In other words, that the same quantity of evaporation should be

* How little information we derive, however, from being told that the absorption of caloric is a consequence of increase of capacity, is easily seen by attending to the meaning of the term. An increase of capacity is a power of combining with a greater proportion of caloric at the same temperature. The whole secret of the explanation therefore consists in this: that solid bodies, in becoming liquid, combine with a greater proportion of caloric at the same temperature, because they thereby *acquire the power* of combining with a greater proportion of caloric at the same temperature.

 produced

produced in the presence of air by a less quantity of caloric than is required in its absence. The contradiction of this, by experiment, is explained by telling us the air, being after a short time saturated with water, acts merely by its mechanical pressure, and that to overcome this pressure the vapour requires the combination of a greater proportion of caloric. Yet, after all, we find that vapour in the formation of which this excessive quantity of caloric has been employed, manifests considerably less caloric in its condensation than vapour produced in vacuo with a heat of 70°.

In what, then, have we reason to believe the action of caloric in evaporation to consist? From what has been already said, and particularly from the experiment just noticed, we have, I think, reason to conclude that no known phenomena authorize us to refer it to any direct specific combination. Were this supposed, it would surely be expected that we should assign some determinate proportion to such combination. If this cannot be done, if we can in no respect so proportion the caloric applied that it shall uniformly produce the same modification, however we may think ourselves able to account for this difference of effect, our explanation must inevitably imply that there is some condition more essential to evaporation than the union of caloric. Thus, as in the explanation of the impediment which evaporation sustains from the pressure of the air, it cannot be pretended that this acts by preventing in the first instance the direct combination of caloric, but by preventing this combination from producing the effects which would otherwise accompany it, we are fully authorized to conclude that the direct combination of caloric is not, as the ordinary theory of this phenomenon, and as all analogy drawn from the phenomena of liquefaction would imply, the determinate specific cause of evaporation.

It becomes interesting, therefore, to investigate that more essential condition of this phenomenon, which the common character of all the circumstances which modify it indicates to be *expansion*: and, since we find that it is by the degree in which this condition is induced, and not by the quantity of caloric actually present that the agency of the latter in evaporation is to be measured, and since we find that expansion has a constant tendency, even independently of caloric, to cause evaporation, we must expect to derive, from tracing the modifications which this condition implies, our most comprehensive ideas of the principles of these phenomena.

Viewing the expansion of a body in its most simple light, merely as indicating a diminution in the attractive tendencies of its particles, there are two ways in which our knowledge of these tendencies suggests to us that it may be effected: one, by a change in the structure or configuration of the particles themselves; the other, by a change in the

* The influence of the air has indeed been an equal subject of perplexity with those who have attempted to associate it with the agency of caloric in evaporation, and with those who have excluded it from this association. If we would believe the most celebrated advocate of the latter opinion, it is alike to the mechanical influence of the air (for he denies it to have any influence but mechanical) that we are to attribute the liquefaction of vapour at one degree of pressure, and its power of resisting liquefaction when subjected to a greater. See De Luc's Theheocalagie, Vol. I. p. 20, &c.

mutual relation of the principles of which theſe particles are conſtituted. And this conſideration inculcates an important diſtinction between two proceſſes, which have been hitherto, if not altogether confounded, ſeparated at leaſt by a diſtinction worſe than none: I mean ſublimation and vaporization. The ſlighteſt attention will ſhow the abſurdity (ſuppoſing it the ſame in both) of calling by different names the action of caloric upon bodies as they are in a ſolid or in a liquid ſtate. Nor would ſo futile a diſtinction probably ever have occurred, had not a real ſpecific difference of action in the two caſes been obſerved to be generally denoted. In the former, nothing but a minute diviſion of the parts (which ſtill retained in other reſpects all their former characters) was obſervable: whilſt, in the latter, there was not merely a comminution of parts, but a total diverſity of appearance. If we ſuppoſe the action of caloric to be in the former proceſs, which comprehends all the ſimpleſt bodies, that of merely *changing the ſtructure and configuration of the particles*, whilſt in the latter it exerts itſelf in *changing the mutual relation of the conſtituent principles* themſelves, ſhall we not have at once an explanation of the phyſical nature of this diſtinction, and a ſatisfactory clue to ſome of the moſt important phenomena of nature?

Before we advert to the evidence of facts, let us ſee how far analogy would carry us in making ſuch a change in the conſtituent principles of compound bodies as a probable account of the action of caloric in their vaporization. As the ordinary proceſſes of diſtillation ſo frequently exhibit to us the ſeparation of the parts of chemical compounds from each other, according to their reſpective degrees of volatility, and as we can underſtand nothing by the different volatility of ſubſtances but their different affinities for caloric, it is obvious that as far as the firſt general dictate of analogy is to be truſted, we are decidedly authorized to expect that all bodies, whoſe conſtituent parts poſſeſs different affinities for caloric, ſhould be liable to the ſame decompoſition. And ſuch an inference can be objected to only on the ground of ſome palpable inconſiſtency with the evidence of facts themſelves. The conſtruction hitherto put upon this evidence has been, that bodies which, in common language, preſerve their conſtitution entire in evaporation, combine with caloric not according to the diſcrete affinities of their ſeveral parts, but according to their aggregate ſpecific affinity for it. How far this aſſertion is warranted by the direct teſtimony of fact will be examined preſently. In the mean time it may be remarked that not only the general analogy juſt noticed, but all the individual analogies alſo, which are any way concerned, unite in contradicting it. According to this aſſertion, the conſtituent affinity of the parts of water (for inſtance) for each other ſhould be ſo great as entirely to ſuperſede the difference of their reſpective affinities for caloric. But this is not at all conſiſtent with the circumſtances which occur in its ſynthetical formation; where foreign aid is always neceſſary to eſtabliſh the ſuperiority of the former; and where the caloric con antity given out during the union of the parts gives us ſtrong reaſon to preſume that its re-combination muſt indicate their diſſolution. Beſides, the mere circumſtance of expanſion accompanying the combination of caloric ſufficiently indicates its tendency to diſſolve in

greater or leſs degree not only the coheſive but the conſtituent affinities of compound bodies *. In fact, if we conſider attentively, we ſhall find that we can form no accurate conception of a change produced in the former which ſhall not affect proportionally the latter. As the coheſive affinity of a compound body owes its exiſtence to the conſtituent affinities of its parts, the idea of a diminution in the former includes in itſelf the idea of a correſpondent diminution in the latter.

Thus, then, we find that every ſuggeſtion of analogy would lead us, *à priori*, to expect that a partial or complete diſunion of parts ſhould always attend the combination of compound bodies with caloric; and, conſequently, that their evaporation is to be regarded as a true decompoſition. In ſubmitting it to the direct teſtimony of facts, however, the phenomena of evaporation will be deemed in two reſpects irreconcilable with the theory which theſe analogies would ſuggeſt. The different parts of ſuch bodies, it will be ſaid, do not eſcape in that ſucceſſion which their different degrees of volatility would thus indicate: nor does their appearance after the proceſs accord with the idea of their decompoſition. With reſpect to the latter, however, to infer that there is no decompoſition, becauſe the ſubſtance collected after evaporation preſents the ſame appearances which it had before, is not an admiſſible deduction: ſince the vapour cannot be ſo collected without having its caloric attached; and as the theory ſuppoſes the combination of caloric competent to the diſunion, it implies equally that the ſeparation of caloric will be effective of the re-union of the component principles.

But there are alſo poſitive facts to be oppoſed to the objection. Numerous inſtances of evaporation occur in which we do ſee evident marks of decompoſition, and of a decompoſition which we can aſcribe to nothing but the combination of caloric. Dr. Prieſtley's experiments on the generation of air from water, viewed independently of the theory which he connected with them, afford ſtrong illuſtrations of it. The ready diſunion of the parts of vapour by many ſubſtances incapable of decompoſing it in the ſtate of water; as well as the indications which Count Rumford has noticed of the decompoſition of water in operations of cooking, muſt be admitted as at leaſt preſumptive proofs. And the combuſtive qualities of the vapour of the eolipile, and ſeveral phenomena of a ſimilar kind, preſent evidences of decompoſition ſufficiently ſatisfactory †. Nor are the evidences of a ſimilar

* I did not know till after I had written the above, that Fourcroy, in ſpeaking of the capacity of hydrogen for caloric, had expreſſed himſelf in the following manner: " It even appears that this baſe, though " combined with oxygen in water, ſtill poſſeſſes the property of abſorbing a great deal of heat. And that " it is this property which renders vapour ſpecifically lighter than air." Diſcourſe on Modern Chemiſtry.

† To theſe may be added the very important inferences to be drawn from meteorological phenomena, reſpecting the decompoſition of water in what is called ſpontaneous evaporation. I muſt acknowledge myſelf indebted for much light on theſe phenomena to a very ingenious paper on the ſubject, in the Eſſays by a Society at Exeter, and I am diſpoſed to differ from the author only on thoſe points in which he ſeems to differ from himſelf. The true tenor of his arguments is certainly to prove that water exiſts in a decompoſed ſtate

ſimilar decompoſition in the evaporation of other bodies leſs conſpicuous. The vapour of ether, according to Lavoiſier's experiments (Mem. de l'Ac. des Sciences, 1780) becomes, by the mere admiſſion of common air, a permanently elaſtic fluid, and in this ſtate deflagrates on the application of flame, like a mixture of oxygen and hydrogen gaſes. The very intereſting experiments of the Dutch chemiſts on the decompoſition of alcohol and ether, in the paſſage of their vapours through tubes of glaſs or of porcelain, may be conſidered as affording evidence to the ſame general purport.

In anſwer to the other part of the objection, that if ſubſtances be conſidered as decompoſed in evaporation, their parts ſhould eſcape ſucceſſively, according to their different affinities for caloric, and not altogether, as is the caſe, it might be ſufficient to obſerve, that the eſcape of one from the remainder in an uncombined ſtate, all our obſervations of other phenomena teach us that this ſtate muſt neceſſarily be that of an elaſtic gas, and therefore the aeriform converſion of both parts muſt take place together. But we may go further, and affirm that it is to the phenomena, as thus explained, that we muſt have recourſe, in order to underſtand one of the moſt important and diſtinguiſhing circumſtances by which the proceſs of evaporation is marked. I mean the generation of cold. And if this can be ſhewn, the theory which I have been attempting to explain will have ſomething more than analogy for its ſupport.

ſtate in the atmoſphere. Yet he ſeems to have formed his idea of it as "a permanent gas, unchangeable, "like other *compounds*, except by an elective attraction ſuperior to that which unites its ingredients," page 368. And in this light we cannot wonder at his expreſſing his inability to explain "what occaſions "the formation of water in the air, to admit of its falling in rain at any time." Without profeſſing to enter at all into the *rationale* of this phenomenon, I may obſerve that the principal difficulties with which it ſeems embarraſſed in his theory will be removed, if we conſider water to enter into the atmoſphere, not as a *permanent compound gas*, but as decompoſed into the two original permanent gaſes belonging to its conſtitution. Theſe, having different ſpecific gravities in the atmoſphere, muſt be ſuppoſed to occupy different regions in the atmoſphere, which, however, will vary, and occaſionally intermingle with each other by the operation of different circumſtances. In ſuch caſes the affinity between their reſpective baſes, when aided by any occurrence diſpoſing them to part with the caloric which ſeparates them, may be eaſily ſuppoſed to occaſion a re-union.

According to this author's idea of the air proceeding from the evaporation of water being a permanent gas, to what can he aſſign its ſpecific diſtinction from water itſelf? Will he ſay in its combination with caloric? But he has ſhown that it is both producible without the aſſiſtance and irreſolvible by the ſubtraction of caloric. Beſides, we have poſitive evidence that this gas, in its original ſtate, is not permanent. The vapour immediately riſing from boiling water is this gas in its original ſtate, tranſparent and elaſtic. But it will not be ſaid that this is incondenſable by cold. It becomes permanent only by the local ſeparation of its parts. And if we admit this ſeparation, I think we not only ſee the arguments which the author has brought together to illuſtrate the modifications of the atmoſphere by evaporation in a ſtronger light, but we ſee the analogy of ſeveral other modifications with the ſame general ſource. We ſee in particular why a greater quantity of oxygen is obſerved in the atmoſphere during dry ſerene weather than during the prevalence of clouds, fogs, or rains; and, connected with this, why the air of lower regions has been always found charged with more oxygen than that of more elevated ones.

It

It requires little attention to discern that the fancied analogy between the cold which is produced in evaporation and in liquefaction, has in reality nothing for its support but the generality of occurrence in each phenomenon. The cold which takes place in liquefaction is a mere consequence of the subject of the liquefaction being surrounded by bodies of a higher temperature than its own, whose caloric therefore is in some part detached, on the general principle of its tendency to equilibrium *. In evaporation, on the contrary, the circumstances are nearly the reverse. The subject of this process has in general a higher temperature than that of the substances around it. The tendency to equilibrium between these will not account therefore, as in the case of liquefaction, for the cold which takes place. We must look forward for the occasion of it to the product of the operation.

We shall be assisted in our examination of this by premising, that we have no instance in the whole physical history of nature of a substance producing cold in consequence of any difference between its capacity and that of the surrounding bodies, unless it have at the same time a temperature actually inferior to their's. I have already shown that the cold produced in the liquefaction of ice is to be referred to this: and we may easily satisfy ourselves that in no frigorific phenomena whatever have we reason to ascribe it to any thing else. Thus in mixtures of salts and snow, it is not the change of capacity which is the proximate cause of the attraction of caloric from surrounding bodies. It is merely the remote cause of it by the reduction which it occasions in the temperature. By this change of temperature only are we able to judge of the nature and extent of the change of capacity, and the accuracy of this idea is clearly demonstrated by the uniformity of its correspondence with the change which we find produced in the freezing point of the new compound.

From these considerations we have a right therefore to infer, that the vapour of water, ether, &c. cannot produce cold in the surrounding bodies without having a temperature actually inferior to their's. But we know that the evaporation of these substances produces cold when the temperature of the surrounding bodies is greatly inferior to that which is universally deemed essential to the subsistence of these vapours. We know that, were it possible for the vapours of water, or of ether, to subsist at the temperature here implied, instead of subtracting caloric from such bodies, they would impart their own to them and undergo condensation. We know also that, if it were possible for the vapour of water (for instance) to subsist at a temperature as greatly inferior to that of 212°, it would have a much greater affinity for the caloric of the evaporating water than the water itself: of course that, whilst such vapour was present, the evaporation could not proceed, which is contrary to fact. We are inevitably led to conclude, therefore, that the cold here produced, is not produced by the vapour (understanding the word in its ordinary acceptation) of the subject of the operation. Nor do I conceive how it is to be accounted for, but by ascribing

* The cold in this case is inaccurately said to be the consequence of change of capacity. This change of capacity merely determines the extent to which the proximate cause, the inequality of temperature, shall operate. This distinction, though as obvious, is not sufficiently attended to.

it

it to that product of the decompoſition of the evaporating ſubſtance, which, having the leaſt affinity for caloric, is rendered gaſeous by the ſeparation of the reſt, before it has acquired ſufficient caloric to ſatisfy its reſumed capacity.

I might have added ſeveral facts of inferior importance to the teſtimony of thoſe already noticed, but I have thought it ſufficient to ſhow that the univerſal dictate of analogy, as to the combination of caloric with ſubſtances whoſe component parts have different degrees of volatility, is ſupported by the evidence of the moſt important phenomena: and that we have reaſon, therefore, to conclude, that in every ſuch caſe a reſumption more or leſs complete of the diſcrete affinities of the conſtituent parts for caloric muſt take place, and that in moſt inſtances, at leaſt of evaporation, this reſumption may be conſidered as entire, and the decompoſition complete. It is certainly not inconceivable that there may be bodies, the conſtituent affinities of whoſe parts may be ſo great as to overcome the tendency of their different affinities for caloric with any heat we can apply. It is not probable, however, that ſuch exiſt among the ſubſtances with whoſe conſtitution we are hitherto acquainted.

I am, SIR,

Your's reſpectfully,

JOSEPH ASTLEY.

Edinburgh, Jan. 31, 1801.

VI.

Account of an Electrometer. By CIT. C. L. CADET *.

THIS author, according to the uſeful cuſtom of the French writers, gives a ſhort prefatory account of the electrometers of *Dufay* with threads, *Henley* with the quadrant and index, *Canton* by the ſucceſſive diſcharge of a jar by ſparks, *Lane* by the length of the ſpark, *De Sauſſure* by the balls of *Canton*, and *Coulomb* by the method of torſion; to which he might have added the floating inſtrument of Nollet, the meaſure of repulſion by weight by *A. Brook*, the extent of electric atmoſpheres determined by the luminous point, by *Magiotto* and *Van Marum*, the inclination of a thread at a given diſtance by the ſame philoſophers, and the gold leaf electrometer by *Bennet*. He has purpoſed to conſtruct an inſtrument capable of application to the meaſurement of ſtrong as well as weak electricities, without requiring any attention to friction or weight. Of this I ſhall give the deſcription and account in his own words:

"Fig. I. Plate 11. In a glaſs tube A, 18 or 20 inches long, is incloſed another ſhorter tube X, ſealed at both ends. This tube contains a graduated ſcale: one of the ends of theſe

* Abridged from the author's paper in the Annales de Chimie XXXVII. 68.

two

two tubes is cemented in a handle of turned wood, C, by which it is held in the hand; the other end is closed by a brass cap, D; the distance between the extremities of the small tube and that of the large one is filled with red wax; B, B, on the cap D is screwed at pleasure, either a ring E, or a brass hook F. The ring is used for applying the instrument to the ball of a conductor, and the hook when it is hung to a ring: on the cap D is a brass stem G, terminating by a knob. This stem is bended, and the extremity of its knob must be directly beneath the line with which the graduated scale of the small tube commences.

Round the large tube is a brass ring H, half of which extends to the length of twelve or fifteen lines, in the form of a half tube P, applied against the sides of the tube. This gutter serves to mark the degrees, by sliding along the graduated scale by means of a button beneath, I. On the ring H is fixed one of the small electrometers invented by Saussure, K, K, which is surmounted by a stem V, on which stem is fixed at pleasure either a point L, or a ball M, of the same size as that which terminates the stem G, opposite which it is placed. The extremity of this point or ball must be placed immediately over the extremity of the half tube or scale P, and horizontally to the centre of the ball, which terminates the stem G.

At the top of Saussure's electrometer is a small ring N, which serves to connect it with the chain Z, when required.

To explain the use of this instrument by a single experiment, I charge a Leyden jar, till the spontaneous overflowing announces it to be saturated. I then place the ring E on the knob of this bottle, and cause the electrometer of Saussure, armed with its point, to slide towards it. I remark the degree at which the divergence of the thread stream commences, and at that instant I suppress the point, and adapt in its place the ball M. I continue to advance the electrometer of Saussure till the electric pressure of the atmosphere in the jar causes the threads to diverge. I again observe the degrees, replace the point L, and close the shutters of my chamber; I then continue to advance the electrometer till the luminous point appears, which again affords new degrees. Lastly, I replace the ball M, and fix the chain Z to the small ring N; I cause it to communicate with the exterior coating of the jar, and advance the electrometer till the explosion takes place. Then comparing the different degrees, I ascertain the comparative difference between the respective methods.

As soon as these relative proportions have been once accurately ascertained by attentive observations, one of those methods alone will be sufficient for measuring the intensity of electricity, and in fact, if the body intended to be submitted to examination be little charged with the electric fluid, the diverging of the threads by means of the point, will fix the limits of the electric atmosphere: if it be more, the pressure of the atmosphere on the ball M, which is substituted for the point, will indicate this quantity. In short, if the body be loaded with a considerable mass of electric matter, it will be shown by the luminous point. If a Leyden jar, instead of being positively, is negatively electrified, the

 point

point indicates it at the same time that it measures the electric atmosphere; for instead of a luminous point, a star will be observed upon the ball of the jar, and another at the end of the point.

Let us now apply this electrometer to useful observations:

In order to connect the idea of a determinate quantity of fluid to each degree of the electrometer, it is necessary to compare these degrees with the known quantities. Suppose for instance I have a jar, the coating of which is six inches square; I electrify it till a spontaneous discharge takes place, and remark, by means of Henley's electrometer, at what degree this discharge is effected. I again electrify the jar, till it is nearly saturated, and measuring with my electrometer, I observe, that the luminous point appears for instance, at two degrees; I then say, that when my electrometer, applied to an electrified body, marks two degrees, the body contains six inches square of electricity. I repeat this experiment with a plate of glass, the coating of which is seven, eight, ten, or twelve square inches, and I form a scale of proportion, which is of the greatest utility in accurate experiments.

In endeavouring to ascertain some of these propositions, I made an observation which has convinced me of the utility of my electrometer in discovering the capacity of electric apparatus. Having taken a jar from an electric battery, I electrified it, and measured it with a point which I passed along a string of silk; on observing the distance at which the luminous point appeared, I joined this jar to another of the same size, and imagined that by doubling the quantity of matter, the measure I had taken would also be doubled; on the contrary, however, the latter measure was not more than about one third of the former: I then added a third bottle, and still obtained nearly the same result; whence the following proposition appears to be established; namely, that the extent of the electric atmosphere is in an inverse ratio to the quantity of fluid accumulated. Another observation which I have several times made, on measuring the electric atmosphere of a conductor, is, that the limits of this atmosphere form an elliptic figure around the body, nearly similar to that represented at Fig. 2.

This, doubtless, arises from the electrified body suspended in a chamber, being nearer to the earth than the cieling; but it would be a curious experiment to measure it at an equal distance from every attracting body, in order to observe whether the fluid has not really a tendency to descend towards the earth, rather than in any other direction. It is my intention to repeat this experiment, as I consider it of great importance to ascertain whether electricity gravitates towards the globe.

From these first attempts, I conceive my electrometer would be well adapted for measuring the absolute capacity of Leyden jars, and also their capacity with regard to their size, or to the quality of the glass of which they are constructed; for the latter, by its greater or less density, absorbs a greater or less quantity of fluid.

VII.

*Inquiries relative to the Experiments of Cit. Prevoſt, of Geneva *, on the expanſive Force of Odoriferous Emanations, and of Cit. Venturi, of Modena †, upon the Motion of Camphor upon Water. By Dr. J.* CARRADORI DEL PRATO ‡.

AS I have been employed perhaps more than any other philoſopher, in obſerving the curious motions of camphor, and all the other oleaginous ſubſtances, as well fluid as ſolid, when caſt on the ſurface of water, and as I flatter myſelf that I have given a true explanation § of theſe phenomena, which had in vain been attempted by Romieu, Volta, Brugnatelli, Lichtenberg, and other reſpectable philoſophers, I was of opinion that it was proper for me to repeat the experiments of Prevoſt and Venturi on this ſubject. I have juſt done this, and find that thoſe of Prevoſt do not poſſeſs that novelty which was at firſt imagined; but on the contrary, the whole, and particularly thoſe of Venturi, tend ſtill farther to eſtabliſh the truth of my explanation.

I have proved ||, that all the fixed and volatile oils, as well as the reſins and concrete volatile oils, for inſtance, camphor, move upon water in conſequence of the attraction which every oil, whether volatile or fixed, has for the ſurface of the water, by which it is attracted, and compelled to extend itſelf till the reſpective attraction of a determinate ſurface is ſaturated; and that there are oils, or oily ſubſtances, which have a greater attraction than others for the ſurface of this liquid. This has been preciſely confirmed by the new obſervations of Prevoſt and Venturi.

The facts related by Cit. Prevoſt are certainly true, as has been previouſly remarked by Cit. Brongniart, but the conſequences which he conceived to reſult from them are not equally accurate.

It is true, as Prevoſt has aſſerted, that if fragments of a concrete odoriferous ſubſtance, for inſtance ſmall pieces of camphor or particles imbued with an odoriferous liquor, be placed upon a plate, or on a piece of wetted glaſs, that is to ſay covered with the ſlighteſt film of water, the latter will be immediately ſeen to retreat from thoſe parts where the ſubſtances have fallen or ſettled, as if it were repelled by ſome force proceeding from thoſe bodies.

But it is not true that the retreat of the water from the abovementioned ſmall bodies is cauſed by the atmoſphere of odoriferous emanations, which proceed from them with impetuoſity, and repel whatever they meet with: but the fact is, as Venturi has already ob-

* Annales de Chimie, Vol. XXI. p. 254.

† Ibid, Vol. XXI, p. 262. See alſo our Journal, Vol. I. p. 153, and 205.

‡ Tranſlated from the Annales de Chimie XXXVII. 33.

§ Ann. di Chimica e d'Hiſtoria natur. di Brugnatelli, tom. v.

|| See my memoir on this ſubject, in the Opuſc. Scelti di Milano. Ann. di Chimic. et Giorn. fiſico-medico di Brugnatelli, and particularly Vol. XVI. of the laſt mentioned work.

ſerved,

ferved, that the oil of camphor, as well as all the other oils, have a greater attraction than water for the furface of the plate. In fact, Venturi obferved, and the fame remark was afterwards made by myfelf, that the part of the plate whence the water has retreated when touched with a fmall piece of camphor, or any body dipped in volatile oil, remains varnifhed with a coat of very fubtile oil, which is perceptible by the eye*; whence he rightly concluded, that the water retires to obey its affinity of aggregation, which becomes free from the attraction of the furface of the plate as foon as that furface is occupied by the oil; and this effect happens only becaufe this affinity of aggregation or cohefion has become greater than the attraction of the water to the plate. This is fo true that the fame phenomenon always occurs, whatever oil be employed, whether volatile and odoriferous, or not, fuch as oil of olives or lintfeed. As foon as the wet plate is touched with the finger flightly dipped in one of thefe oils, the water retreats in a circle, as if it were touched with any odoriferous body.

There is doubtlefs, as Cit. Venturi has obferved, a ftronger attraction between the oil and the furface of the plate, than between the latter and the water; which is evidently proved by the obfervations: let a drop of olive, or any other fixed oil, be dropped on a fmall plate of porcelane, glafs, or any kind of glazed earth; and afterwards pour fome drops of water from a certain height, which on uniting together quickly flow like a fmall torrent over the oil; it will be feen, that notwithftanding the force with which this fmall torrent ftrikes the drop of oil, it is not capable of difplacing it and carrying it off, but paffes over while the oil adheres to the furface of the plate, like a varnifh, which the impetuous and continued paffage of the torrent cannot remove. It is well known that plates, or other culinary utenfils, cannot be cleaned by water when foiled by oil or fat, as it has not force enough to detach the oily particles from their furface, though it can produce this effect with earth, and diffolve extractive fubftances.

If a fmall piece of camphor be fixed upon a plate or other veffel, and as much water be added as will flightly cover the camphor, the water, as Prevoft has obferved, can never attain to a level above the camphor, but forms a fmall cavity or gulph, in the figure of an inverted cone, in which the fummit of the piece of camphor is always perceptible; this evidently proves, according to him, that there proceeds from the odoriferous body an elaftic fluid of fufficient force to repel the water that furrounds it, which according to the laws of hydroftatics, ought otherwife to come in contact with and cover the camphor.

Cit. Venturi has likewife obferved this phenomenon; but he conceives it to be the effect of an oily fluid which proceeds from the camphor, and prevents the adhefion of the water with the piece of camphor, and with the columns of water that furround it.

For my own part, after the moft accurate examination, I am of opinion, that the retreat of the water is neither attributable to the emanation of an elaftic fluid, to an odoriferous vapour, as Prevoft has fuppofed, nor to the obftacle that prevents the adhefion of

* Loco citato, p. 207.

water with the camphor, produced by a subtile oil proceeding from the camphor itself; but that it is owing merely to the cohesion between fat or oily bodies and water. Oils possess much adhesion or attraction of surface* with water; but they have no cohesion or affinity of aggregation, nor of composition with that liquid. The water which surrounds oily or fat bodies, such as a piece of camphor, having no degree of cohesion with that substance, appears to retreat from it; that is, by abandoning itself intirely on its own force of aggregation it is drawn off on all sides, which causes a vacuity round the camphor. If it were an elastic odour, or an oily fluid proceeding from the camphor, which produces this phenomenon, the same effect would not take place in bodies which have no smell. But the phenomenon is the same, when instead of camphor, small pieces of white wax or hardened suet are employed, which are substances as every one knows, which have scarcely any smell, containing an oil which is not volatile, and requiring a considerable degree of heat to become fused.

Hence it appears, the experiments of Prevost do not prove that odoriferous bodies are surrounded with an atmosphere possessing a degree of elasticity or power capable of impelling the water which surrounds them; and that it has been falsely imagined that by this means the emanations from odoriferous bodies might be rendered perceptible.

I did not observe, what Prevost says he remarked, that fragments of camphor about the size of a pea, placed on a round piece of gold or silver foil floating upon water gave it motion. The camphor itself is not susceptible of any spontaneous motion, nor can it impart it to light bodies which are on the water, unless it immediately touches that liquid. It is the same with regard to the oils, whether volatile or fixed; they cannot impart the least degree of motion to the lightest body that floats upon water unless they touch it. I have made several trials, by putting drops of ethereal or volatile oils on small pieces of gold or silver leaf that floated upon water. The slightest impression would have caused them to move, yet I never observed the least motion in any of them. Venturi also had the same result in an experiment with burning or heated camphor, placed on a piece of cork which floated upon water. When the camphor came in contact with the fluid, it imparted a rapid motion to its support, which, on the contrary, became motionless when the water was not touched by the camphor. Hence he concluded, that the motion in question does not depend on the projection of volatile particles proceeding from the camphor, but that it is the water alone which excites it by its action.

Prevost and Venturi remark, that if the surface of the water be touched with oil of olives, or any other volatile oil or odoriferous fluid, when the camphor begins to move, the motions immediately cease. This only tends to confirm the explanation I have given of the motion of the camphor and oleaginous bodies on the surface of water. The camphor derives its motion only from the expansion of an oil, which is attracted by the surface

* See my explanation in *Ann. di Chim.* Italiani, tom. V.

of

of the water *. The reason therefore why a drop of oil, whether fixed or volatile, stops the motion of the camphor, is, that these oils are more strongly attracted by the surface of the water, than by that of the camphor; hence they adhere to that surface, and by covering it prevent the effect of its attraction for the oil of the camphor, and consequently prevent the expansion of that fluid †. This property of preventing the motion of the camphor is not peculiar to the oils; all the cereal and leguminous farinæ, &c. the juices of the tithymalæ, and other lacteous plants, which, as I have observed in the memoir before-mentioned, have a great affinity for the surface of the water, are invested with similar properties.

Cit. Venturi has nearly discovered the real explanation of the phenomenon. Having observed that a small quantity of oil stopped the motion of camphor in a bason of water, he concluded that this oil, which extends over the water like an imperceptible film, is the cause of the suspension of motion in the camphor, because, by occupying the surface of the water, it prevents the camphor from expanding. Whence he seems to be of opinion, that the motion of pieces of camphor upon water proceed from the expansion of an oily substance from the camphor upon the water itself. This is the explanation which I have given of the above-mentioned, and other similar phenomena, in several memoirs published previous to the year 1794.

Cit. Venturi however, appears in some degree to follow the opinion of Prevost, since he asserts that the volatility and odoriferous property of camphor are not qualities requisite to produce the revolving motions which this substance exhibits when cut in small pieces and cast upon water; but that its volatility is nevertheless necessary for the continuance of such revolutions.

The motions of camphor, as I have proved in the memoir so often quoted, and which is inserted in the xxth vol. of the "*Opuscules choisis de Milan*," depend on two causes, *viz.* the extension of an oily fluid over the surface of the water, and on the rapid operation (evaporation) of this fluid itself.

When camphor cut in small pieces is thrown on the surface of the water, the instant they touch this fluid they begin to move in a circular direction, and run with rapidity in different directions; but shortly afterwards their motions decrease, and the particles collect in small heaps, which, though they have not an absolute, have nevertheless what may be called an intestinal motion. All this takes place in consequence of the consumption of the particles of camphor which form the small heaps; for according to the former remarks of *Lichtenberg*, as they necessarily change their figure, they are compelled almost every instant, being subject to follow the attraction of the mass, which varies the contacts according to their diversity of figure. The water, by the attractive power of its surface causes the extension of the volatile oil of the camphor. This oil, by exposing a great su-

* Opusc. Scelti di Milano, tom. XX.

† Vide my Memoir "*Sulle attrazion di superficie*," Journal des Phys. et Ann. di Chimic. Italiani, tom. XVII.

perficies to the air, evaporates more ſpeedily, and as its evaporation is in proportion to its extent upon the water, the reaſon is evident why the camphor is ſo ſpeedily conſumed, and why by this rapid conſumption the particles are induced to change their figure.

It is true that the oil camphor, which is extended over the water, gradually evaporates or entirely volatiliſes. Of this I have been clearly convinced by the following experiment: after two pieces of camphor which had been in motion on the ſurface of water in a glaſs were conſumed, I expoſed the glaſs to the air, that the oily film of oil camphor which covered the ſurface might evaporate; four hours afterwards I put in other pieces, but did not obſerve the accuſtomed winding motions; I ſoon removed them, and at the expiration of four hours I again threw in freſh pieces; and the motions took place as uſual. Hence it appears, that after four hours the oil of the camphor had not totally evaporated, though in eight hours it had diſappeared, and the water remained perfectly clear.

Venturi, however, was well acquainted with the attraction that exiſts between oil and the ſurface of water; ſince he obſerves that a drop of oil, if it had no affinity with the ſurface of water, would remain upon it without extending; it may therefore be ſaid, that an attraction exiſts between the ſubſtance of oil and the ſurface of water.

The experiments made by Venturi with ſmall columns or ſticks of camphor, plunged half their height perpendicularly in water, and cut horizontally at the level of that fluid, in the ſpace of twenty-four hours, only proves that the greateſt ſolution of camphor is effected exactly where it touches the ſurface of the water; and conſequently they tend to confirm my concluſion, that camphor contains an oil which expands over the ſurface of water in conſequence of an attraction between it and this ſurface; and that it is by ſuch extenſion that it more ſpeedily evaporates. There is not, as he imagined, a greater activity exerted upon the camphor at the points where the air and water meet in contact with this ſubſtance, but only where the camphor touches the ſurface of the water. Air only conduces to this influence upon the phenomena, in proportion as it favours the evaporation of the oil of camphor which is extended over the ſurface of the water. I am well convinced that air here has no influence, and that the whole effect ought to be attributed to the union of the ſurface of the water.

But it appears that Venturi has at length diſcovered his error, ſince he ſays, he obſerved that the ſolution of camphor at the ſurface of water takes place the quicker the more that ſurface is extended. For in ſmall veſſels the cutting of the columns is not ſo quickly effected.

Prevoſt has alſo confirmed the attraction of the ſurface of water; as he obſerved that camphor when in a humid air, or ſubmerſed in water, does not undergo ſo much loſs or conſumption, as it ſuffers when it immediately touches the water; but he attributes this to the water, which favours the evolution of the odoriferous fluid.

The rapid motion of odoriferous bodies upon water, and particularly that of camphor, is prevented after ſome time, as Cit. Prevoſt has obſerved, in conſequence of the attraction of the ſurface of the water for the oil which expands being diminiſhed: nevertheleſs, the ſmall pieces of camphor which collect on the water after their revolving motion has ceaſed,

always

always retain a flight degree of motion, by which they refemble groupes of capillary eels, that move fpontaneoufly, till they are entirely confumed by evaporation: this motion depends on the flow expanfion of the volatile oil of camphor which takes place at the furface of the water, in proportion as that which remains is evaporated, as we have already mentioned.

At firft I entertained doubts whether the motions of the camphor upon water were fufpended by the expanfion of olive oil, or any other oleaginous fubftance, merely becaufe that of the camphor, being more denfe, did not poffefs the faculty of expanding as quick as the liquid oils. To elucidate this point, I made the following experiments: I melted a fmall quantity of camphor in a fpoon by the flame of a candle, and poured it upon hot water, on the furface of which a drop of olive had previoufly expanded: in its melted ftate it was not at all capable of extending, and expelling the olive oil; ftill lefs of giving motion to farinaceous matter. I next threw fome pieces of the inflamed camphor on the furface of cold water; and they moved extremely well. As I fuppofed that it was the heat of the inflammation, which by melting the oil of the camphor obliged it to continue to expand, I poured on this water fome drops of olive oil; the motion of the camphor then immediately ceafed, and did not recommence, though the fubftance continued to burn. Hence it muft with certainty be concluded, that the oil of camphor has lefs attraction of furface for water than olive oil, or feveral other oleaginous fubftances, fuch as the flour of wheat.

Camphor, well heated, and fuming, as Venturi has obferved, being brought near very light bodies floating on the furface of water, fuch, for inftance, as pieces of gold or filver leaf, exerts upon them a repulfive force, exactly the fame as when it is thrown upon cold water; and, according to him, this effect is owing to the elaftic fluid, which ftrikes againft thofe bodies: this notion alfo occurred to Prevoft. For my own part, I think this effect is produced only by an oil which expands upon the water: the oil of camphor, reduced to vapour by heat, when it comes in contact with water, expands on the furface, and thus imparts a motion to the bodies it meets with, and feems to repel them. In fact, if we attentively obferve the furface of the water around the floating body which is moved, it will be feen to be covered with a fmall very thin and whitifh film (of varnifh) which is, doubtlefs, a proof that the oil of the camphor, when volatilifed, is extended over the water, and by cooling depofits a fine incruftation.

It is not, therefore, proved by any of thefe experiments that the atmofphere, which is formed by the odour of the bodies, poffeffes, as Prevoft pretends, an expanfive power capable of being rendered perceptible. Thofe bodies which are moft effectually odorant, that is to fay, furrounded, like the others, by an atmofphere of odoriferous emanations, but which are not mixed with a volatile oil, do not caufe any repulfion in bodies floating in water, however near they may be brought to each other.

And if the mere emanation and projection of an elaftic odoriferous fluid were the only caufes of the motions of camphor and other odoriferous bodies by water, thefe motions would take place on throwing in any one of fuch bodies; but if, for example, even the fmalleft piece of camphor be dropped upon water with a confined furface, the motions are not produeed; it is therefore evident that they are caufed by the furface of the water alone.

Account

VIII.

Account of the original Invention of the circular Inſtrument for performing Calculations on the Principle of Gunter's Rule made by Clairault le Père, and communicated to the Royal Academy of Sciences in the Year 1727*. (W. N.)

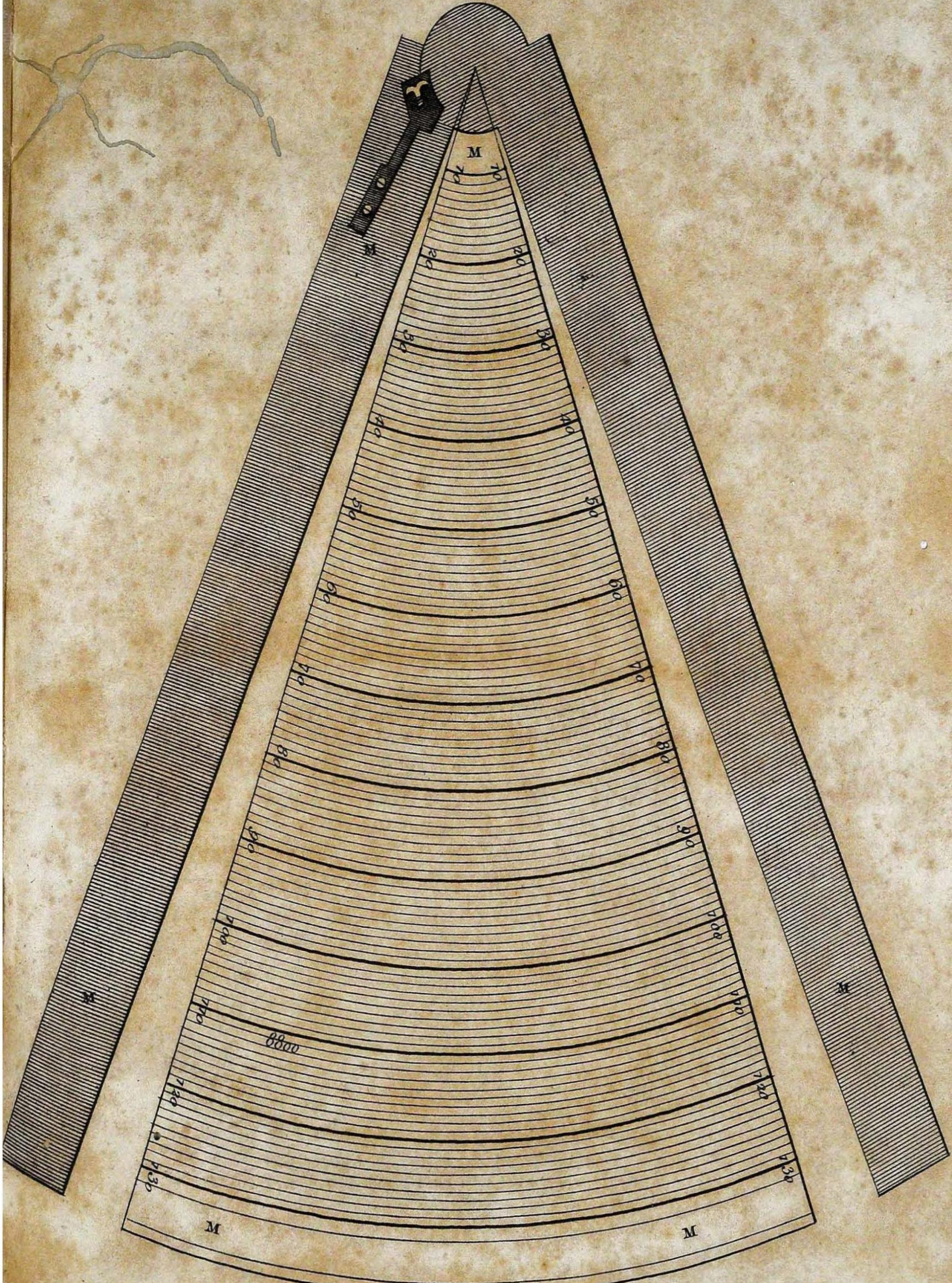
IN the firſt volume of this Journal, page 372, I gave an account, with drawings, of a ſpiral inſtrument conſiſting of a combination of the ſector and Gunter's rule, equivalent to an ordinary rule of forty feet in length, which had been communicated by me to the Royal Society about thirteen years ago, as my own invention. In the paper here referred to, I did juſtice to George Adams the father, who, as I afterwards diſcovered, had contrived and made a very powerful inſtrument of the ſame kind in 1748, ſince preſented to me by his ſon. The ſame principles of candour and juſtice call upon me at preſent to aſcribe the original thought, as well as the actual mechanical conſtruction, to the elder Clairault; of whoſe engraving, publiſhed twenty-one years before the date of Adams's manuſcript, I have here given a copy. An abridged account of his paper will ſhew that he was well aware of the properties and advantages of this apparatus.

He begins by obſerving that the beſt expedients for the ſolution of right lined triangles are reducible to linear conſtruction, or logarithmic calculations; that the firſt, by means of the ſector, requires much ſkill, to produce a moderate degree of accuracy: while the latter demands a certain quantity of reſearch and computation, which render it deſirable to ſimplify the proceſs. With this intention he propoſed to unite theſe two excellent diſcoveries which he performed by placing all the logarithms diſtinctly on the ſurface of a circle of twenty-one inches in diameter, upon concentric circumferences; marking not only the natural numbers as far as ten thouſand, but alſo the ſines of the degrees and minutes. The figure in plate 1 repreſents a twelfth part of the circular inſtrument, and the external ſhaded branches denote the ſector between which the intervals are meaſured. In the year 1716, the author executed his firſt ſyſtem on a ſquare of one foot, filled with parallel lines, conſtituting altogether a rule of 1500 French feet: and it was not till the year 1720 that he thought of the ſpiral form. But as this ſet of lines, when numerous, preſented ſome difficulties, principally, as I ſuppoſe, from the difficulty of marking and counting the intervals upon the blades of the ſector, he gave the preference to concentric circles. I need not enter into the minute particulars of the conſtruction, becauſe this has been already done in our Journal, and in the Philoſophical Tranſactions. He does not overlook the advantage (which alſo occurred to me) of this ſtructure that it affords by the enlargement of the external arcs, a remedy for the ſmallneſs of the differences in certain parts of the logarithmic lines.

The learned writer concludes his paper by giving ſome numerical examples of its uſe and application.

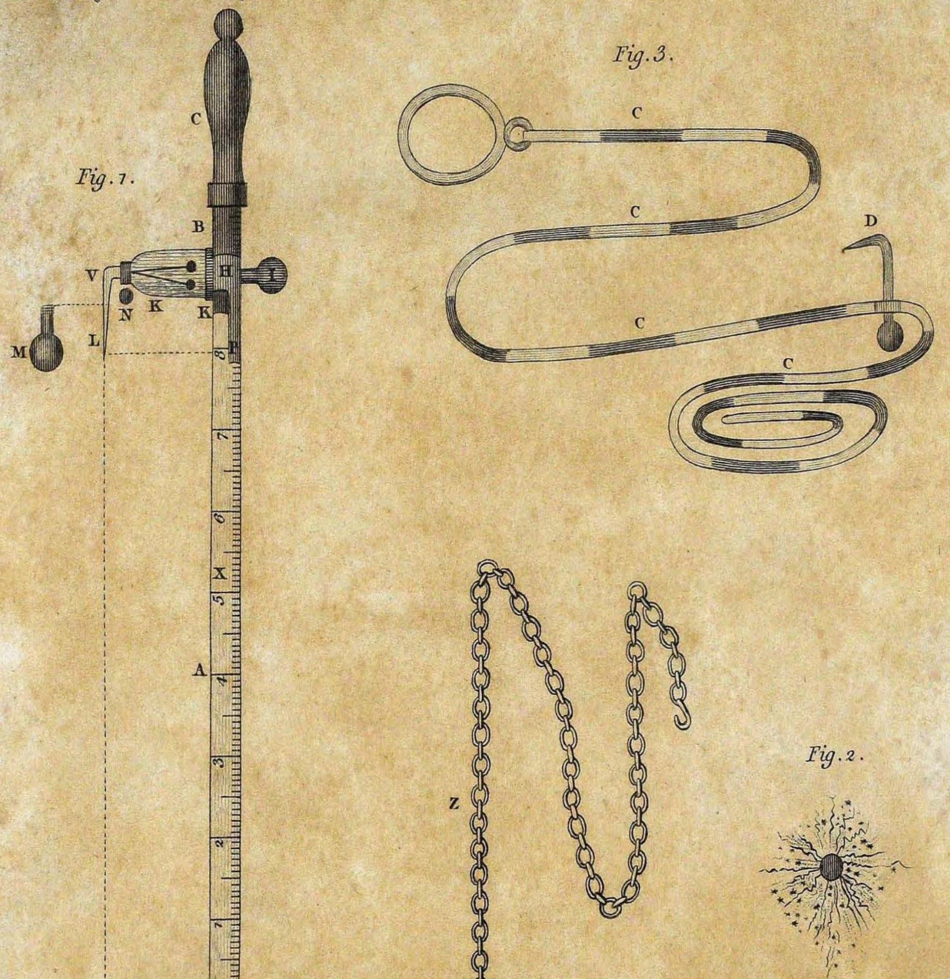
* Machines approvées par l'Acad. Roy. tome V.

M. Clairaut's Instrument for Logarithmic Computation.



Mutlow Sculp.

Electrometer of Cit. Cadet.



Mutlow Sculp.